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CLINICAL LECTURES

ON THE

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BY THE LATE

ROBERT J. GRAVES, M.D., F.R.S.,

PROFESSOR OF THE INSTITUTES OF MEDICINE IN THE SCHOOL OF PHYSIC IN IRELAND.

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TO THE

RIGHT HONORABLE WILLIAM, EARL OF ROSSE, K.P.,

President of the Royal Society,

THIS, THE SECOND EDITION OF A TREATISE

ON

CLINICAL MEDICINE,

IS RESPECTFULLY DEDICATED BY HIS FRIEND,

THE AUTHOR.

ADVERTISEMENT.

41, Grafton Street,
Dublin, *January,* 1864.

The Clinical Lectures of the late Dr. Graves have been for some time out of print; and the constant demand for them has been such as to induce the publishers to issue this reprint of the second edition, edited, in Dr. Graves' lifetime, by the late Dr. Neligan.

They have much pleasure in annexing, from the *Medical Times and Gazette*, the following translation of the Introduction to the French edition of this work, from the pen of Professor Trousseau, of Paris :—

"TO THE TRANSLATOR.

"Sir and honored Confrère,

"For many years I have spoken of Graves in my clinical lectures; I recommend the perusal of his work; I entreat those of my pupils who understand English to consider it as their breviary; I say and repeat that, of all the practical works published in our time, I am acquainted with none more useful, more intellectual; and I have always regretted that the Clinical Lectures of the great Dublin practitioner had not been translated into our language.

"As Clinical Professor in the Faculty of Medicine of Paris, I have constantly read and re-read the work of Graves; I have become inspired with it in my teaching; I have endeavoured to imitate it in the book I have myself published on the Clinique of the Hotel Dieu; and even now, although I know almost by heart all that the Dublin professor has written, I cannot refrain from perusing a book which never leaves my study.

"Graves is an erudite physician; while so rich in himself, he borrows perpetually from the works of his contemporaries, and at every page brings under tribute the labours of German and French physicians. Although a clinical observer, he loves the accessory sciences; we see him frequently have recourse to physiology, in the domain of which he loves to wander; to chemistry, with which he is acquainted, which he estimates at its true value, and to which he accords a legitimate place. He often reminds me of the greatest clinical teacher of our day, Pierre Bretonneau, an able physiologist, a distinguished chemist, a learned botanist, an eminent naturalist, who incessantly in his lectures and conversation at the Hospital of Tours, found in all those accessory sciences, with which he was so conversant, those useful ideas and ingenious views which he subsequently applied with unusual felicity to the study of our art.

"Shall I now say what are, in Graves' work, the most remarkable and important lectures? To be just, I ought to indicate all in succession: there is not one of them, in fact, which does not abound in practical deductions; there is not one which does not bear the impress of the admirable and powerful faculty of observation which distinguishes the physician of the Meath Hospital. The lectures on scarlatina, paralysis, pulmonary affections, cough, headache, have acquired an European reputation, and the interest with which they inspire every attentive reader is assuredly their best panegyric.

"There are, however, two points to which it is important to call particular attention. Graves has devoted a great many lectures to typhus fever, which so cruelly decimates Ireland. It might be supposed, at first sight, that the study of this portion of his work is not of so much importance to us, French physicians, who, fortunately, have not to contend with the formidable malady in question. This is a mistake. All the precepts of the author upon the treatment of this pyrexia are so applicable to the severe forms of our typhoid fever, that we shall with the greatest advantage consult this remarkable work. Moreover, the maxims relating to regimen have become the guide of the practitioners of all countries: it is they which now direct us in the treatment of putrid fever. And, nevertheless, when he inculcated the necessity of giving nourishment in long-continued pyrexias, the Dublin physician, single-handed, assailed an opinion which appeared to be justified by the practice of all ages; for low diet was then regarded as an indispensable condition in the treatment of fevers. Had he rendered no other service than that of completely reversing medical practice upon this point, Graves would by that act alone have acquired an indefeasible claim to our gratitude.

"On the other hand, I cannot sufficiently recommend the perusal of the lectures which treat of paralysis; they contain a complete doctrine, and this doctrine has decisively triumphed. The sympathetic paralyses of Whytt and Prochaska have now their place assigned in science, under the much more physiological name of reflex paralyses, and the Dublin professor is the first who has studied with exactness their etiological conditions, as he is the first who has made known their pathogenic process. Anticipating by many years the admirable works of Marshall Hall, he has comprehended, he has seen that anomalous peripheric impressions may react upon any section of the medulla, and determine at a distance disturbance of movement or of sensibility; he has, in a word, created the class of peripheric or reflex paralyses, and he has clearly established the relations existing between these paralyses and acute diseases.

"Unhappily, these remarkable lectures have remained a sealed letter for the majority of French practitioners; but it is time to render to the physician of the Meath Hospital the justice which is due to him; it ought to be known that Graves is the creator of this new doctrine, which has profoundly modified within a few years the pathology of the nervous system; it is right, in fine, to refer to its true author the suggestive theory of the paralyses and the convulsions of peripheric origin.

"You have then, sir, done a very useful work in publishing Graves' Lectures. You have rendered a great service, if not to beginners—who will perhaps not find in them the elementary ideas which are necessary to them—at least to physicians, who must understand the reasons of instinct and intelligence by which they ought to allow themselves to be guided in the difficult paths of practice; who are called upon to assist in the doubts, embarrassments, and perplexities which trouble the conscientious man when he is engaged in those obscure cases which so frequently present themselves in the wards of an hospital.

"Graves is often empirical. What true clinical observer can avoid being so? But he is so only in spite of himself. He seeks, he points out the reasons which determine him; he discusses them, and he conducts his pupil, step by step, from the theory, occasionally too ingenious, to the application, which is always useful though often unexplained.

"Graves is a therapeutist full of resources. For the majority of French physicians his medications present something unusual, because the agents he employs are rather less used in France; but we learn in his lectures the medicine of our neighbours at the other side of the Channel—a medicine strange to us, as ours is to them. We learn in them the methods most relied upon in the United Kingdom, and the remedies to which our English colleagues give the preference. I freely confess that I had some difficulty in accepting, notwithstanding the imposing authority of Graves, what he states of the influence of certain remedies, such as mercurials, essence of turpentine, spirituous preparations, nitrate of silver, &c.; but the Dublin professor speaks with so much conviction that I ventured to follow his precepts, and I must say that my early trials very soon encouraged me to adopt unreservedly what at first I accepted only with misgiving. There is not a day that I do not in my practice employ some of the modes of treatment which Graves excels in describing with the minuteness of the true practitioner, and not a day that I do not, from the bottom of my heart, thank the Dublin physician for the information he has given me.

"Graves is, in my acceptation of the term, a perfect clinical teacher. An attentive observer, a profound philosopher, an ingenious artist, an able therapeutist; he commends to our admiration the art whose domain he enlarges, and the practice of which he renders more useful and more fertile. We shall, therefore, all be much indebted to you, my dear confrère, for having rendered familiar to us an author unfortunately too little known among us.

"A. TROUSSEAU."

THE EDITOR'S PREFACE.

HAVING, at the request of Dr. Graves, undertaken to edit the present edition of his work on Clinical Medicine, my chief aim has been to improve its truly practical character, and thus render it if possible more useful to the profession. With this view I have altered and re-arranged the Contents, classifying the various diseases and subjects treated of, and throwing the entire into the more suitable form of lectures. This, so far as related to the Second Part—which in the first edition consisted of miscellaneous essays—I found but little difficulty in doing; for the author having been always in the habit of dictating to a shorthand writer, his style naturally assumed a colloquial character, and therefore required but very little alteration to reduce it to that of a Lecture.

With this same object in view, whatever alterations or additions I have myself made, I have incorporated with the text; knowing practically the great inconvenience and distraction of mind to the reader, which editorial notes or matter inserted between brackets produce. Moreover, I have been differently circumstanced from most other editors, having had all through the zealous co-operation of the author and his approval of the alterations and additions made.

The reader will perceive that I have introduced into this Edition several of the author's essays which were omitted from the first: of these I wish to call especial attention to his observations on two subjects—the Pulse and Cholera. The greater part of the former, which now constitutes the fourth lecture, was originally published in the Dublin Hospital Reports nearly five and twenty years since, and contains an account of the first *accurate* experiments which were made as to the effects of posture on the frequency of the pulse;—an inquiry which has been since then carefully investigated by Knox, Guy, and others, with the effect of stamping with correctness the original observations of Dr. Graves, and proving their practical value.

The subject of the Cholera is just at present an all-important one, when this pestilence is ravaging a great portion of the globe, and those countries which have been once and but once before afflicted with it are again threatened with a visitation. Shortly after the cessation of the previous epidemic, Dr. Graves read an essay before the College of Physicians on its origin and progress, chiefly with the view of proving its *contagious* character; this essay, which was published at the time in the *Dublin Journal of Medical Science*, is now remodelled, and a short history of the present epidemic, as far as it had advanced at the time those lectures were going through the press added.

The lectures on Fever, which constitute so large and so valuable a portion of the first volume,* will be found to be much altered as regards arrangement; and the causes and mode of diffusion of the late epidemic with which this country was visited have been noticed.

Although many years have elapsed since several of the author's views on the physiology, pathology, and treatment of diseases were first published, and the science of medicine has been since extraordinarily advanced by the aid of the chemist and the histologist, but few alterations or omissions have been required to adapt them to the present state of knowledge. Indeed, it is singular how many of his observations, which, when first promulgated, were from their novelty either doubtingly received or altogether rejected, have been corroborated by the investigations of more recent inquirers. Of these I would particularly notice his views "on the Capillary Circulation, and on the

---

* In perusing this and the Author's Preface, the reader is requested to observe that the second edition was in two volumes.—[*Pub. of Reprint.*]

Doctrines of Inflammation," confirmed by the most recent microscopical investigations; "on the Circulation of the Blood within the Cranium," confirmed by Dr. Burrowes' experiments; "on the Pathology of Paralysis," so remarkably in accordance with the Cerebro-spinal Reflex Theory; "on the Pathology of Tubercle;" and "on the Nature of the Acid in the Human Stomach."

In conclusion, I have only to add that these volumes, as now presented to the reader, contain the results of Dr. Graves' additional experience during the five years which have elapsed since the first edition was published.

J. MOORE NELIGAN.

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THE AUTHOR'S PREFACE.

THIS Work first appeared in 1843, and its publisher informed me last year, the sale had been so rapid that he expected the whole edition would be soon disposed of. The event more than justified his anticipations, and consequently he requested me to prepare a second edition for the press, a request I felt bound to comply with, particularly as I was conscious that much might be done to render the work more deserving of the approbation which my brethren and colleagues in all parts of the world had so kindly, and to me so unexpectedly, bestowed on it.

On revising the volume as before printed, I detected so many faults and errors, that I at once resolved to remodel the whole, and accordingly I applied myself to the accomplishment of this object, with a sincere desire to render my Clinical Medicine still more useful to the profession. I soon found, however, that my task was a very difficult one. The original work contained so much which a maturer reflection and experience disapproved of, that the sections to be omitted soon swelled to a formidable bulk; while, on the other hand, a closer review of the matters discussed suggested the necessity of inserting many lectures that had been formerly left out.

The occupations of a laborious profession so encroached on my time, that I found my plans could not be executed, without associating myself with some other physician, in whose industry, learning, and ability I reposed confidence. Having been fortunate enough to secure the co-operation of Dr. Neligan, I felt certain that the result would be satisfactory, and I placed in his hands the numerous cases I had collected from my own practice, and the various extracts I had made from books since the publication of the first edition. To these materials Dr. Neligan made many and important additions, and he has bestowed so much labour on the two volumes now submitted to the profession and public, that I feel confident this edition will be found a great improvement on the former.

ROBERT J. GRAVES.

Merrion Square,
September, 1848.

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CLINICAL LECTURES
ON THE
PRACTICE OF MEDICINE.

LECTURE I.

CLINICAL INSTRUCTION.

GENTLEMEN,—Before we commence an examination of the cases at present in the medical wards of this hospital, it is necessary to explain the method of instruction which I mean to adopt. Employed elsewhere in learning the principles that constitute the basis of medical education, you ought to be impressed with a precise notion of the peculiar objects and utility of hospital attendance. You come here to convert theoretical into practical knowledge; to observe the symptoms of diseases previously known to you only through the medium of books or lectures; to learn the art of recognising these symptoms, and of appreciating their relative importance and value; to study their connexion with morbid alterations of internal organs; and, finally, to become acquainted with the best method of relieving your patients, by the application of appropriate remedies.

Such, gentlemen, are the objects you seek in coming here; and in proportion to the number and importance of these objects, are the degree of responsibility attached to your clinical instructors, and of blame to yourselves, should the opportunities which this institution offers for your benefit be neglected.

The other branches of medical education may be cultivated at different times, and according to a certain order of succession,—one period of your studies demanding a particular application to anatomy, another to chemistry, while a third must be especially devoted to materia medica. With the observation of disease it is otherwise. From the very commencement, the student ought to witness the progress and effects of sickness, and ought to are not very numerous here, for this hospital possesses no other attractions, confers no special qualification beyond the knowledge which may be obtained within its walls.*

Of those who are anxious to learn their profession, a great number fail, and are found wanting when their studies are finished; in a few, the failure may be traced to a deficiency of intellectual powers; but in the majority it is owing to their studies being erroneously directed. Thus I have known many who have displayed a taste for the study of the progress and treatment of acute diseases, while they paid but little attention to complaints of a chronic nature. This predilection is not confined to students; professors and authors in general seem to participate in this taste; and, consequently, we find that acute diseases form the favourite subjects of clinical lectures, and occupy the greatest portion of medical literature—and for obvious reasons; for if the course of acute diseases, such as fever and the phlegmasiæ, be compared with that of chronic maladies, we shall find that the former begin, continue, and end in a manner comparatively so regular and definite, that their progress can often be accurately predicted, and their terminations foreseen,—a circumstance which enables us not only to predict the event with confidence, but obtain, by the well-timed application of active remedies, relief, evidently the result of the means employed, and, consequently, reflecting credit both upon the physician and the art of medicine. How satisfactory are our feelings on arresting the progress of pneumonia by venesection, or tranquillizing the mania of delirium tremens by means of opium!

Far different is the case with chronic diseases; in their commencement generally obscure, insidious, and irregular; in their terminations necessarily uncertain; frequently transferring themselves, as it were, from one part of the system to another, occasioning unexpected and anomalous symptoms, and involving in their destructive course almost every tissue of the body.

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* Since this was written, the Meath Hospital became for several years a privileged hospital. Latterly this premium upon idleness has been again withdrawn from us, and I most heartily rejoice that this and other hospitals have ceased to form a sort of favoured oligarchy to the exclusion of the less extensive institutions of this city; everything like monopoly tends to retard the advancement of science, and I see no reason why an hospital with 50 beds should be inferior to one with 100. It is not the quantity of disease a teacher treats which renders his lesson instructive; his diligence and accuracy of observation are the best means of instructing the pupils.

From the very length of their duration, they are also more liable to be modified by new physical and moral influences, affecting either the mind or body; and are, in a word, more closely leagued with time, the parent of mortality. In the treatment of such affections, the greatest judgment and patience are requisite; there is here no room for the application of *heroic* remedies; nor can the physician expect, from his most persevering exertions, that speedy benefit by which he acquires *éclat* in acute cases, for it must be remembered that chronic diseases require chronic remedies.

This most difficult department of medicine surely claims not the least portion of your attention, and you will attach more importance to this subject, on considering that a knowledge of chronic diseases is essential to the surgeon, inasmuch as those who labour under them remain exposed to accidents which constitute his peculiar province.*

Many students fail from another cause: instead of studying the most common, and, on that account, the most important diseases, they acquire a taste for observing and relating singular and rare cases, as if their chief object was to obtain a store of curious medical information. Let me warn you against this amusing, but comparatively unprofitable employment of your time. Suffer not yourselves to be misled by those who prefer the gratification of an idle curiosity to the laborious investigation of ordinary diseases.

Students should aim, not at seeing many diseases every day, not at visiting daily numerous cases; no, their object should be constantly to study a few cases with diligence and attention; they should anxiously cultivate the habit of making accurate observations. This cannot be done at once; this habit can be only gradually acquired. It is never the result of ability alone; it never fails to reward the labours of patient industry. You should also endeavour to render your observations not only accurate but complete; you should follow, when it is possible, every case from its commencement to its termination; for the latter often affords the best explanation of previous symptoms, and the best commentary on the treatment. Did time permit, I could expose many other erroneous practices calculated to render your studies comparatively unprofitable; but I must turn from the student to the teacher—from the errors of the learner to the imperfection of the mode adopted for instructing him.

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* At the time this lecture was written, the absurd idea that the education of a surgeon should differ from that of physician had not been altogether abandoned.

I have had an opportunity of observing with attention three different methods of conducting clinical instruction; the first is that practised in Edinburgh and Dublin. I shall select that of Edinburgh for examination, being by far the most celebrated of the British schools of physic, and much resorted to even by foreigners for instruction.* Two clinical clerks, one for the male, another for the female wards, are selected by the physician from among the senior pupils; their business is to write an accurate history of the cases, to report the effects of medicines, and record the symptoms which may have occurred since the physician's last visit. All this is generally done with fidelity and zeal. At his daily visit the physician stops at the bed of each patient, and having received the necessary information from his clerk, he examines the patient, interrogating him in a loud voice, while the clerk repeats the patient's answer in a tone of voice equally loud. This is done to enable the whole audience to understand what is going on; but indeed, when the crowd of students is considerable, it is no easy task; it requires an exertion almost stentorian to render this conversation between the physician and his patient audible by the more distant members of the class; while the impossibility of seeing the patient obliges all who are not in his immediate vicinity to trust solely to their ears for information.† This information is not indeed neglected, for every word so attentively listened to, and heard with so much difficulty, is forthwith registered most faithfully in each student's case-book; and afterwards all the observations the professors make in their clinical lectures are taken down with equal care and fidelity.

It is really a pity to find so much labour and diligence thrown away; for it is evident that the practice of medicine cannot be thus taught or learned, as it were, by hearsay; and it is consequently to be feared that many are annually dubbed doctors at Edinburgh who have been scarcely ever called on to write a prescription. The chief objection to this mode of teaching is, that however well inclined the student may be, he is never obliged to exercise his own judgment in distinguishing diseases, and has no opportunity of trying his skill in their cure; and, consequently, at the end of his studies he is perhaps well grounded in the accessory sciences—is a perfect medical logician—able to arrange the names of diseases in their classes, orders, and different subdivisions; he may be master of the most difficult theories of modern physiologists; he may have heard, seen, and, if a member of the Medical Society, he may have also talked a great deal; but at the end of all this preparation, what is he when he becomes a full doctor?—*a practitioner who has never practised!*

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* I speak of Edinburgh as it was when I studied there in 1819.

† When this information was conveyed, as it formerly was at Sir Patrick Dun's Hospital, in Latin, the student had to encounter another barrier to the acquisition of knowledge. I have called the *language* LATIN, in compliance with the generally received opinion concerning its nature.

I do not assert that a diligent student may not obtain a good deal of knowledge by attending one or several clinical courses in Edinburgh; no doubt he will gain many useful general ideas concerning the nature and treatment of disease; and if he himself examine the patient after the physician's visit, he may even acquire a certain degree of tact in recognising symptoms and appreciating their value. This method of instruction is indeed very useful, and nothing better can be devised for a beginner; but for the more advanced student it is by no means sufficient, nor is it calculated to give him practical experience, without which all other acquirements are of no avail. I say it does not give him *experience*, because he has at no time been charged with the responsibility of investigating a case for himself and by himself, because at no time has he been called on to make a diagnosis unassisted by others, and above all, because he has never been obliged to act upon that diagnosis, and prescribe the method of treatment. If those who have been thus educated, and who have been made doctors upon so slender a foundation, were to confess the truth, we should be presented with a picture calculated to excite dismay, if not a stronger feeling. How many doubts and distracting anxieties attend such a man at his first patient's bedside? If the disease be acute, and life in imminent danger, and if he shrink under this sudden and unusual load of responsibility, he gains little credit for professional ability; if, on the contrary, inexperienced as he is, he assumes that decision of judgment, that energy of practice, which experience alone can confer, it is not improbable that the result may be still more disastrous.

Gentlemen, I am not drawing a picture from my imagination alone; I have had occasion too often to shudder at the original,—too often to deplore the sad effects resulting from the well-meant but totally mistaken treatment employed by young men; and often have I regretted that, under the present system, experience is only to be acquired at a considerable expense of human life. There is, indeed, no concealing the truth, the melancholy truth, that numbers of lives are annually lost in consequence of mal-treatment. The victims selected for this sacrifice at the shrine of experience generally belong to the poorer classes of society, and their immolation is never long delayed when a successful candidate for a dispensary commences the discharge of his duty. The rich, however, do not always escape; nor is the possession of wealth in every instance a safeguard against the blunders of inexperience.

This charge of inexperience is not necessarily confined to the beginner; it applies equally to many an old practitioner, whose errors have grown, and have increased in strength, during a long succession of years; because, from a defect in his original education—from the absence of a properly directed clinical instruction, he commenced practice without having previously acquired the power or the habit of accurate observation; because he had not in his youth been taught to reason justly upon the facts presented to his view; because, not having learned in the beginning to think accurately, he contracted a loose and careless mode of examining the progress of disease and the effects of remedies; and, consequently, the lapse of time has had no other effect upon his errors than that of rendering them more inveterate. Such a man has generally an overweening confidence in his own judgment; he never detects or is conscious of his own mistakes; and, instead of improvement, years bring only an increased attachment to his opinions—a deeper blindness in examining the results of his own practice; and do not such persons abound in every branch of the profession?—are there not general practitioners, are there not physicians, are there not surgeons, are there not apothecaries, who answer to this description, and who nevertheless are cheerful in their demeanour, and enjoy a good repute among their clients? Believe me, gentlemen, the quacks who cover our walls with their advertisements, vend not annually to the community more poison than is distributed according to the [illegible] of [illegible] and licensed practitioners;— and [illegible] of medicine is improving daily, and treatises [illegible] every day multiplying. Why, then, [illegible] Many circumstances concur to produce [illegible] the most influential is undoubtedly that which [illegible] attention. I mean a system of clinical instruction [illegible] because it does not teach the actual [illegible] other profession or art, or [illegible] would embark un- [illegible] knowledge." But enough of [illegible] consider what systems [illegible] with a view of judging how [illegible] them also [illegible]

[illegible] is very [illegible] is com- [illegible] are defects. [illegible] to the [illegible] also pre- [illegible] these [illegible] morning, more than two hours upon eight or ten cases, besides the time devoted in the evening to the same purpose. When the importance of the subject to be taught is so great, it is wisely judged that the teachers must be laborious; and it is thought necessary to use every possible means to convey clear ideas concerning each case to the student. His attention is not distracted by seeing a great number of cases in rapid succession, nor (as is too often the case in the hospitals of Dublin and London) are the inquiries dictated by a laudable curiosity on the part of the student suppressed by a forbidding demeanour or an uncourteous answer from his teacher.*

Although the French clinic thus presents several manifest superiorities over the British, yet it is liable to the chief objection already urged against the latter—that the student is not supplied with an opportunity of learning the actual practice of his profession. I am by no means disposed to join in the cant of humanity; yet I cannot overlook another disadvantage to this mode of teaching. I cannot help feeling that it is scarcely justifiable to lecture upon a patient's case in his presence, and in his native language; that it is cruel to explain (as must, when this method is adopted, be often done) that the patient is labouring under a fatal complaint. During such a lecture I have often watched the worn and pallid countenance of the sufferer, while he listened attentively to the record of his past and present sufferings, and I have marked the settled expression of despair it assumed when the prognosis thus tediously ushered in, was too clearly announced. It is cruel to banish from the sick man's bed his sole remaining comfort; it is unmerciful to scare away hope—his only consolation during hours of pain and watching. We ought never to allow any expression to escape from us which could possibly add the terrors of apprehension to the weight of actual suffering. On this account, while we borrow the useful part of their system from the French, we must correct so glaring a defect by making use of the Latin language, whenever it is absolutely necessary to make any observation that might alarm the patient.* One of the most important duties of a surgeon, or physician, consists in the practice of humanity; and it is very doubtful whether the student does not experience as much difficulty in deriving benefit, not so much from the precept as the example of his seniors, in this department of his profession as in any other.

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* In this respect our hospital physicians and surgeons have improved much since 1821. I am strongly disposed to believe that the improvement was not owing to a voluntary change, but to a certain salutary fear of public castigation from the weekly medical press; much, however, remains to be done, for the influence of the last century has not yet entirely ceased, and there are those still lingering among us, who no doubt regret the aristocratic era when an impassable gulf lay between the student and his teacher.

Observe, gentlemen, I speak not of French, but of Irish hospitals; for, with the exception of the objection already adverted to, the conduct of the French medical men is in every respect praiseworthy. We do not find them indulging in coarse, harsh, and even vulgar expressions to their hospital patients; we do not find them provided with two vocabularies—one for the rich, and another for the poor.† The medical, more than any other profession, requires that the better feelings of our nature should be cultivated and fostered. The nature of anatomical pursuits obliges us to violate many of our natural prejudices, and disregard some of our strongest propensities; let us therefore be doubly anxious to give, by means of the most diligent cultivation, an additional and more vigorous growth to our better feelings—to our social affections; and if we are accused of disrespect for the dead, let us answer the accusation by our humanity to the living.

But to return to our subject. The third mode of conducting clinical instruction is that adopted generally throughout Germany; and which, in addition to the means of improvement comprehended in the plan of the French and English methods, possesses the advantage of allowing the more advanced students to undertake the care of patients in the hospital, under the direction of the attending physician.

The importance of clinical instruction is so much felt in

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* This rule is always observed in Germany, a country remarkable for the zeal and humanity of the medical profession. In Italy both professors and students are less scrupulous. Thus Dr. Clark relates that he has heard the case of a phthisical person explained in all its bearings by the professor of Bologna, in the patient's presence: in another instance, which occurred at the same place, a female, labouring under cancer uteri, burst into tears on hearing a detailed account of the nature of her complaint.

† When the above lecture was delivered, the abuse I speak of was but too frequent; and will it be credited, that many other and greater abuses had existed during the preceding generation? Death, the most efficient of all reformers, had then removed several of the chief actors from the scene, for which, as on most other occasions, he has, I rather think, been undeservedly censured.

Germany, that each school has three distinct medical clinics attached to it, by which means the labour of teaching is divided among the professors, and the number of students attending each is diminished. There is one clinical hospital for the treatment of acute diseases, and another for chronic diseases, while a clinical dispensary is devoted to the care of extern patients. The pupils are divided into two classes—the more advanced, who get the care of patients; and the junior students, who merely look on and listen. When a patient is admitted, his case is assigned to one of the practising pupils, who, when the physician is visiting the ward, reads out the notes he has taken of the patient's disease, including its origin, progress, and present state. This is done at the bedside of the patient; and before he leaves the ward, the physician satisfies himself whether all the necessary particulars have been accurately reported by the pupil. After all the patients have been thus accurately examined, the professor and his class proceed to the lecture-room, and a list of the patients and the practising pupils is handed to the professor: the cases admitted that day are first inquired into, and the pupils are examined concerning the nature of their diseases, their probable termination, and the most appropriate method of treatment, each student answering only concerning the patients entrusted to his special care. During this examination, the pupil's diagnosis and proposed remedies are submitted to the consideration of the professor, who corrects whatever appears to be erroneous in either, and then the student retires to write his prescriptions, while the rest of the cases and pupils undergo a similar examination. At the conclusion, the prescriptions written by the students are read out in order by the professor, who strictly comments on and corrects any inaccuracy or inelegance they may contain. When the prescriptions have been revised and corrected, they are signed by the physician, and handed to the apothecary to be made up and distributed. In some clinics, the price of each medicine is affixed to the bottle or box containing it, in order that the students may become acquainted with the comparative expense of various prescriptions, and may thus be enabled, in private practice, to accommodate, as far as possible, the expense of the remedies to the circumstances of their patients. The clinic for extern patients is conducted on the same principles: patients who are able to

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attend, are examined at the dispensary; those who cannot leave their homes, are visited by the senior practising students, who always seek the advice of the professor when the case is urgent or the treatment doubtful.

Nothing, gentlemen, can be better adapted than this plan of clinical instruction for the improvement either of the beginner, or of the more advanced student; this daily deliberation and anxious discussion concerning the nature and treatment of each case is peculiarly interesting, and serves to accustom the beginner to habits of accurate examination, whereby he is taught to interrogate nature for himself, and learn the history and treatment of disease, not from books and descriptions, but from direct observation. The advantages gained by the practising pupils are too obvious to require comment: being obliged to give reasons for every plan of cure that they propose, they are accustomed to a rational and careful investigation of disease; and enjoying the most important of all advantages—the early correction of their errors—they commence private practice with a sufficient degree of experience to render them unlikely to commit any very serious mistakes.

It is evident that, according to the German method, no regular clinical lectures are necessary, as the pupil becomes accurately acquainted with the physician's views of each case, and no step is taken in the treatment without the reasons for it being given. This is the best sort of clinical lecture; the pupils have their doubts solved, and their erroneous views corrected, while the professor is enabled to mention, as the disease proceeds, everything which he thinks illustrative of its nature.

Eleven years' experience, since I first delivered the foregoing observations, enables me strongly to recommend the method of instruction pursued in Germany. Since my appointment to the Meath Hospital I have had extensive opportunities of observing its good effects. Not a session has elapsed without furnishing proofs in its favour. This system, however, at first met with much opposition, and its introduction was ridiculed in every possible manner; even now it may be doubted whether its well-wishers are as numerous as might be expected. It is still opposed by several narrow-minded persons, whose opinions have much weight with the pupils.

I remember perfectly well having only two practising pupils in one class, but I was not discouraged; and although we have had many numerous classes in the Meath Hospital, I doubt if any of them contained more talent and worth than was shared between my two pupils, Dr. Townsend and Dr. Stokes.

Since the latter, from being my pupil, has become my colleague, he has evinced the most indefatigable zeal in co-operating with me in instructing the pupils of the Meath Hospital; and I am sure he joins me in testifying the constant gratification we have received from observing that our efforts have been so far successful, that no season elapses without bringing under our immediate observation several pupils whose diligence, zeal, and moral worth insure our warmest approbation. Many of these gentlemen have already distinguished themselves, and will always carry with them the best wishes of myself and my colleague.

Six and twenty years have now elapsed since the foregoing part of this lecture was delivered in the old Meath Hospital, and my subsequent experience has amply verified the opinions therein expressed. I regret to say that however influential these opinions may have proved in this city, their promulgation has produced but little benefit in causing any alteration in the mode of instruction pursued in the medical schools of the United Kingdom at large. So far indeed from the mode of conducting medical education being improved, it has decidedly been altered for the worse. This assertion may appear paradoxical, nay almost incredible, when it is recollected how many new Universities and Schools have arisen since the year 1821, and how many novel medical professorships have been founded.

But if we carefully examine into the instructions given, and the qualifications required in the first and most recently organised medical school of the day, viz., that of the London University, it would readily appear that a very small part of the student's time and attention is directed to acquire a knowledge of how disease is to be actually treated and cured—unless, indeed, we admit that a knowledge of Greek and Latin, of mathematics, algebra, and optics, of physics, botany, and chemistry, is necessary for this purpose. That this multiplicity of subjects distracts every student, is sufficiently evident *a priori*. And my own experience, from opportunities as a public teacher for many years, has satisfactorily convinced me that the practical parts of medicine are not taught so well now as formerly.

It is not intended to assert that pupils now hear fewer clinical lectures, or attend a shorter time in the hospital, but it may be confidently affirmed that what they hear in these lectures, or see in the hospital, does not rivet attention or excite reflection now as formerly. For the pupil's avocations are so numerous, that he is hurried from one to the other, and has no time to devote to serious reflections upon what he has seen.

In Edinburgh, the engrossing subject of conversation amongst students used to be the nature of the diseases of the clinical patients, and the effects of remedies employed; the clinical ward afforded constant themes for discussion, and its contents were constantly before the thoughts of the student. Such was Edinburgh in 1819, how it may be now, I cannot tell; but be it changed for the worse, which I hope is not the case, it must result from a change in the system, and not a deterioration in the professors, whose unwearied diligence in the promotion of medical science daily brings forth fruit not unworthy of the best era of their predecessors.

When so many seductive subjects are successively placed before the student, it cannot be expected that he will think almost exclusively on what is practical. On the contrary, the chances are that the chief energies of his mind will be misspent on the fascinating experiments and doctrines of chemistry, electricity, magnetism, and the polarization of light, to the exclusion of the less fascinating but all-necessary subject of disease and its treatment. In truth, the very rapid advances in the so-named collateral sciences have, of late years, seemed to render the practical improvement of the student less probable, and every day it becomes more unlikely that he will attain to the simple goal that he ought to hold in view, but will be diverted from the pursuit of the one indispensable object by the very means which he is taught to believe are necessary for its attainment. To this subject I shall recur in the following lecture, concluding this with an expression of satisfaction that since the first publication of my views upon medical education, they have been brought forward and enforced in several leading articles by the able editor of the *Medical Gazette*; and they have had, I have reason to hope, a favourable effect upon the manner in which medical education is conducted in my native city.

LECTURE II.

PRELIMINARY EDUCATION.—MODERN NOMENCLATURE.—LIEBIG'S THEORIES.

Having now explained the advantages of this, the German mode of clinical instruction, I shall content myself with remarking that we have had many years' experience of its beneficial effects in the Meath Hospital, where it was introduced by myself in 1821; I must remind you, however, that even its utility is necessarily proportioned to the diligence of the student. There is no system capable of communicating information to the indolent; every man must depend chiefly on his own assiduity, and all the teacher can do is to facilitate the means of acquiring knowledge, and afford an example of punctuality and attention. I would seriously recommend everyone who undertakes the management of cases, to set out with a fixed determination to persevere throughout the whole session. Few things give me more concern than to find young men, who have commenced with ardour, becoming by degrees less and less industrious, until their hospital attendance degenerates into an irksome task, imperfectly performed, and at last wholly neglected. One of the most valuable things which the student can acquire, is *a habit of daily diligence.* The knowledge requisite for the efficient discharge of our professional duties is not to be acquired by sudden starts of intense application, or by the overwrought strivings of desultory exertion; it demands a daily and hourly attention, a steady, constant, and accurate course of observation, continued uninterruptedly for years.

I think students are very much misled as to the best mode of becoming good practitioners. This is an age of ambitious acquirement, and professional men seem to be ashamed unless they have the character of universal knowledge. Everybody studies everything, and the consequence is that few know anything well. We live amidst the din of declamations in favour of general education; and are everywhere assailed by the ceaseless competition of those who vend cheap knowledge in the form of penny periodicals, lectures innumerable, and hosts of rival encyclopædias; but ours is not an age of calm unpretending acquirement and severe precise study, without which the effort to become good physicians and surgeons must prove vain and fruitless.

Can anything be more embarrassing than the multitudinous array of studies presented to the young student who comes to London or Dublin with the view of educating himself as a general practitioner? So many departments of knowledge are spread before him, and so numerous are the exhortations to study each with particular care, that he feels at a loss where to begin. The merits, advantages, and necessity of his own branch, are insisted on by the respective teachers, with all the force of impressive eloquence; and after running the round of introductory lectures—an initiatory penance duly performed by all beginners—he returns in the evening to his home, puzzled and dispirited. He finds that it will be necessary for him to become an excellent botanist, an able and scientific chemist, and a profound anatomist; that he must have some knowledge of zoology, be well versed in comparative anatomy, know how to detect poisons with accuracy, and study the legislative enactments which bear on questions of medical jurisprudence. Physiology, materia medica, therapeutics, nosology, morbid anatomy, the principles and practice of surgery, medicine, and midwifery, claim, all and each, his especial attention; nay, many teachers insist upon the necessity of his becoming master of several languages—Greek, Latin, French, and German; while others assure him that he never can prosecute scientific medicine with success, unless he studies physics as well as physic; some are there even who encourage him to cultivate mineralogy and geology, as if forsooth a knowledge of these sciences could teach the laws that regulate diseased action, or the indications which should govern the exhibition of remedies. In a lecture published by Mr. Hayden, I find it remarked "that to keep pace with the modern race of intellect, we should get on a railroad of literature; mathematics, natural philosophy, the art of drawing, and, above all, logic, will be indispensable." Dr. Elliotson would no doubt add metaphysics, animal magnetism, and phrenology, sciences he has cultivated with success, and taught with perspicuity! Dr. Latham, who has had sufficient courage to put forth his opinions on this subject, has demonstrated, with much truth and force, the injustice and folly of attempting to impose so many burthens on the minds of students, and has shown clearly the bad consequences resulting from such a mode of proceeding.

No profession requires a sounder preliminary education than ours, and in none ought education be more studiously directed to promote the activity and development of the mental powers, especially those connected with *the habit of observation* as well as with *the judgment and memory*. The latter faculty should be cultivated from the earliest period, and the boy should be taught the chief anatomical names, as those of the different parts of the muscular, nervous, and vascular systems, which names he will of course find no difficulty in retaining when a man, and it will then be only necessary to learn the qualities of the things to which they belong. If, in addition to this, boys were taught the scientific names of the chief articles of the materia medica, and the technical terms and classifications of botany and chemistry, much trouble would be saved them in after life; and their memories, while in the state of greatest activity, would be much better employed than in attaining the rules and terms of syntax, prosody, mythology, and ancient geography.

I would not recommend any one to commence the actual study of medicine and surgery until the age of nineteen. Before that period the mind is not sufficiently ripe for practical observation, nor sufficiently stored with that knowledge—only to be gained by the daily intercourse of life—which teaches us to estimate the effects of moral or physical causes on the human system, imparts to us the power of weighing conflicting evidence, and detecting the too frequently incorrect and erroneous statements of our patients. *A certain knowledge of the world* is indispensable to the physician; and it is only loss of time—yes, of precious time—to employ boys in trying to learn what can only be acquired by men. Those who attend hospitals at too early an age are very apt to acquire careless habits of observation; all the interest which disease presents, when observed for the first time by matured minds, is lost to them, and all the attractions of novelty have ceased long before they possess that tact and experience which enable the adult to understand the meaning of symptoms, the progress and phases of morbid phenomena, and the effects of therapeutic agents.

It is then the duty of parents, guardians, teachers, and all who superintend the education of youth, to see that those who are destined for the medical profession should have their minds prepared and strengthened by diligent cultivation during early youth, not only by the attainment of extra-professional knowledge suited to their means and opportunities, but also by instruction in those portions of anatomy, materia medica, botany, and chemistry, which may be readily comprehended at that age. Especial care should be taken to impart to them some knowledge of the physical qualities of medicinal substances. All this being done, when the student, arrived at maturer years, comes to grapple with the practical departments of his profession, he will find many difficulties easily surmounted, and at this period he should disengage himself from too devoted an attention to the accessory sciences. But he need not wholly detach himself from them; some one of them may be cultivated along with his more serious pursuits. He may devote one session to lectures on chemistry, another to those on botany, a third to physiology, and so on of the rest. But his main object must now be the acquisition of practical knowledge, and consequently the greater portion of his time and energies must be devoted to the clinical wards and dissecting-room of an hospital, to the study of materia medica and pharmacy in an apothecary's shop, and to practical anatomy.

Five or six years' attendance on an hospital will be little enough to qualify you to enter with propriety and confidence on the discharge of your professional duties. Bear in mind, gentlemen, that when you come to treat disease, you approach the bedside as physicians or surgeons, and not as chemists, botanists, or anatomists. This is the character in which you are to appear; and, to the acquisition of knowledge which will prepare you for the discharge of its duties you ought to apply your chief attention.

Some of you, gentlemen, may think that it ill becomes a teacher to narrow the limits of your exertions, or circumscribe your pursuits. But let me be understood. What I wish to impress upon your attention is, that you ought to address yourselves mainly to the acquirement of what is really useful, and should store up chiefly what is most important and available.

And in furtherance of this object I think it my duty to warn you against the well-meaning but injudicious representations of those who would turn you from the study of practical matters to the cultivation of their favourite sciences—sciences connected with and ancillary to medicine, but in which medical students are too often encouraged to engage with an ardour that indirectly but certainly leads to a less zealous and efficient attention to more important matters. Take, for instance, two of the most popular of the adjunct sciences—two usually regarded as most intimately connected with the study of medicine—botany and chemistry. Both are extremely valuable in themselves, and a certain acquaintance with them is undoubtedly desirable; but to the student in medicine their utility has been greatly overrated. Botany is an extremely interesting and useful science; but I believe you might be very good practitioners without knowing the classes of Linnæus, or the families of Jussieu. To be sure, if you had the misfortune to practise in localities separated from the ordinary channels of commerce; if you were suddenly bereft of the numerous stores which maritime enterprise pours into the lap of medicine, and obliged, like the herbalists of old, to search the woods and fields for your materia medica, you would certainly be often at a loss, and might make some serious mistakes, unless you were adepts in practical botany. But this labour, fortunately for us and for every European practitioner, is quite unnecessary. A small capital will bring the vegetable productions of the most distant countries to your door; and any respectable druggist will for a trifling sum provide you with all the medicinal substances derived from plants, carefully selected, and accurately prepared.

Those who boast the most loudly of their acquisitions in botany, and who lay most stress on its importance, know very well that to the physician it is of little or no practical value. Take one of the best of our English or Irish botanists, and see how meagre a knowledge he possesses, after all, of many of the plants whose products are employed so largely every day in the treatment of disease. Transport him suddenly to the East or West Indies, to Africa, or South America, ask him to show you the camphor or the cinnamon-tree, the cajeput, the croton, or the guaiacum: I doubt very much whether he would be able to recognise logwood, or even ipecacuanha, growing in their natural situations. Again, there are a great many vegetable productions used every hour in medicine, of which it may be said that no two botanists are agreed as to the precise description of plant from which they are derived. There is no substance in such common use as gum arabic, and yet, notwithstanding all that has been written on the subject, it is not clear from what particular plants it is derived. Nor do I think it necessary to know whether the gum we use in compounding a cough medicine comes from the Acacia vera or Acacia arabica. In like manner, the plants which furnish cardamoms and many other substances in common use are by no means determined. How many disputes have there been with respect to the genus cinchona? And what has been the result of all our investigations concerning the plant which produces this great remedy? Listen to what my late learned friend Andrew Duncan says, in the supplement to the Dispensatory: "Notwithstanding that all the British colleges agree as to the botanical species of cinchona from which the commercial varieties of bark are derived, there is no satisfactory evidence that they are right; on the contrary, it is almost certain that in regard to some of them they are wrong." How many years were calumba and many other similar productions employed, before scientific botanists knew anything of their true history? In 1829 a paper was read by Dr. Hancock, on the tree which yields the Angostura bark; it appears that even Bonpland and Humboldt had described the wrong tree, and consequently it has been called for many years a *Bonplandia;* whereas it belongs, it now appears, to another genus, named Galipea. Dr. Hancock has also proved that the Smilax syphilitica of Wildenow is not the true sarsaparilla, but that it is obtained from other plants: and at what conclusion does Dr. Hancock, who spent many years in South America, arrive? Why, that the only criterion for knowing good sarsaparilla is its taste when chewed! In proof of the uncertainty which still prevails concerning the determination of species used in medicine, I have only to refer you to the admirable lectures of Mr. Pereira in the *Medical Gazette,* and those of Dr. Sigmond, published in the *Lancet.**

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* In the number of the Quarterly Review for June, 1842, we find some very pertinent observations upon the ridiculous names given to many flowers, and the inconveniences likely to arise from the frequent changing of them.

The Reviewer says, "Before we have done with the florists and botanists, we must say one word about their nomenclatures. As long as the extreme vulgarity of the

I do not wish to undervalue botany as a part of general education. Few sciences are more attractive, and few are more likely to become an object of enthusiastic pursuit; but it is the very enthusiasm it is so likely to generate that I wish to warn you against. Botany is an excellent exercise for the minds of youth: it gives habits of accuracy of observation, and tends to strengthen the memory. It leads to healthy occupation, and affords a source of innocent enjoyment. As productive of so much good, let it form a part of the education of young persons in general; sure I am that its cultivation would give a healthier tone to both mind and body, than is to be obtained from many of the studies with which boys are now tortured in the schools. But let botany be restricted within its proper limits; and when once young men have seriously engaged in the acquirement of medical and surgical knowledge, let them not entertain the ambition of becoming accomplished botanists.

Speaking of botany, I may observe that it is much to be regretted that the names of plants should undergo so many

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one and the extreme pedantry of the other continue, they must rest assured that they will scare the majority of this fastidious and busy world from taking any great interest in their pursuits."

After objecting to many modern names, he adds, "Surely there is marked character enough about every plant to give it some simple *English* name, without drawing either upon living characters or dead languages. It is hard work, as even Miss Mitford has found it, to make the maurandias, and alstræmerias, and escholtzias—the commonest flowers of our modern gardens—look passable even in prose: they are sad dead letters in the glowing description of a bright scene in June. But what are these to the pollopostemonopetelæ and eleutheromacrostemones of Wachendorf, with such daily additions as the native name of iztactepotzacuxochitl icohuejo, or the more classical ponderosity of Erisymum Perofskyanum?

"Like the Verbum Græcum
Spermagorsiolekitholakanopolides,
Words that should only be said upon holidays,
When one has nothing else to do.

———— "To make confusion worse confounded, our botanists are not satisfied with their far-fetched names; they must ever be changing them too. Thus it is a mark of ignorance in the world of flowers, to call our old friend Geranium otherwise than Pelargonium; the Glycine (G. sinensis), the well-known specimen of which, at the Chiswick Gardens, produced more than 9,000 of its beautiful lilac, laburnum-like racemes from a single stem, is now to be called Wistaria; the new Californian annual Ænothera is already Godetia; while the pretty little red Hemimeris, once a Celsia, is now (its third designation) an Alonsoa; and our list is by no means exhausted. Going on at this rate, a man might spend the morn of his life in arriving at the present state of botanical science, and the rest of his days in running after its novelties and changes. We are only too glad when public sanction triumphs over individual whim, and, as in the cases of Georgina proposed for Dahlia, and Chryseis for Escholtzia, resists the attempted change."

mutations. What was formerly called Stilozobium has successively become Dolichos and Mucuna; while Iceland moss has been changed from Lichen into Cetraria, and Secale cornutum into Acinula clavus. Uva ursi is now preceded by the prænomen Arctostaphylos; and our old acquaintance jalap, deprived of its euphonious prefix Convolvulus, has degenerated into Ipomæa, still further converted, more recently, into Exogonium. All these changes are useless or injurious, and entail, as a necessary consequence, that the young, the middle-aged, and the advanced in life, use a different medical vocabulary. The materia medica too, as now taught by scientific professors, presents a serious stumbling-block to students. Teachers do not confine themselves to showing the different drugs and preparations, but they enter into very minute details of their natural history and characters; so that the student cannot learn the properties of bees' wax without being entangled in the difficulties of entomology, or the nature of isinglass, without learning the hard names used in ichthyological classification.

The same observations apply to chemistry. It is a science fully as attractive as botany, and medical men are apt to spend too much time in its pursuit. Some very pertinent observations on this subject have at different periods appeared in the *Medical Gazette*, to which I refer you: they are conceived in a spirit of good sense and sound judgment, and you will find them well worthy of an attentive perusal. I grant that it may appear very like a paradox to say, you need not know much practical chemistry. But if you go to a reputable druggist with money in your pocket, he will furnish you with all the chemicals you have need of, excellent in their kind, and prepared with scrupulous exactness. So far as chemicals are required for medicinal uses, you can have them all of the best description. But it will be said, that without an accurate and extensive knowledge of chemistry you cannot prescribe. This is an assertion to which I cannot assent. A very limited knowledge indeed of chemistry will enable you to ascertain what substances are compatible with each other, and a small share of attention will prevent you from making any important mistakes. Besides, you are all aware that many of our best prescriptions contain incompatible ingredients; and that many compounds, which would be sneered at by the mere chemist as heterogeneous and absurd, prove decidedly efficacious in medicine. Granting that a certain degree of chemical knowledge is requisite, it does not follow that you should be scientific and accomplished chemists. It is not necessary that you should dive into all the arcana of the science, or have your memories loaded with atomic numbers, symbols, and equivalents.

Let me repeat with respect to chemistry what has been already observed concerning botany. Students should attend one or two courses of this science as preparatory to the study of medicine, and during the period of that study they may attend another in order to keep up and improve their knowledge; but they should never allow chemistry to cause them to absent themselves from the hospital for a single day. Theoretical and philosophical [illegible] less than animal and pharmaceutical chemistry.

[illegible]

So far with respect to a knowledge of chemistry as connected with the choice and prescription of medicines, or the analysis of poisons. As to any benefits derived from analytical chemistry in solving the problems of vital action, or elucidating the functions of the various organs in health and disease, they may be said to be few and unimportant, and inconclusive. Few and scanty, indeed, are the rays of light which chemistry has flung on the vital mysteries. I am not aware that it has revealed any of the master secrets of the organism, or detected the sources of those important aberrations from normal action which we are called on to study every day and every hour. Chemistry has failed most remarkably in revealing the arcana of life; and notwithstanding all her boasted discoveries, we are still very little in advance of those who practised the healing art some centuries ago. Chemists, the ablest of their class, have bestowed the most minute and unwearied attention on the analysis of fibrin, and gelatin, and albumen; and what have they discovered? Simply this: that substances so apparently distinct in their vital relations, and so different, or even opposed, in their physical properties, are analogous compounds; that there is scarcely any difference in their elementary composition; and that their atomic constitution is nearly identical. How long have chemists laboured in attempting to detect the cause of animal heat! How many experiments have been made for the purpose of ascertaining the effect produced on the air by respiration! How many able and ingenious men have sought a chemical explanation of the difference in point of colour between arterial and venous blood! All these investigations have proved indirectly useful, but none of them have revealed the secrets sought; and we are still in profound ignorance of the powers which direct and modify the unceasing operations of the laboratory over which *life* presides—that mysterious influence, which, like the Deity from whom it emanates, is invisible, inscrutable, incomprehensible.

So much for the light which chemistry has shed on the vital actions, and on the nature of organised compounds. There are, to be sure, one or two instances in which a rough examination of some organic products is necessary—as, for example, of the urine, in certain cases of gout, gravel, and dropsy. But even in these instances a few simple rules will suffice, and sufficient information may be obtained by one moderately acquainted with chemistry. Generally speaking, the chemical knowledge requisite for the study of disease is very limited; and those who are engaged in the practice of medicine are well aware, that cases demanding an accurate or extensive knowledge of chemistry are of extremely rare occurrence.

Let me now advert to a serious inconvenience which the chemists have imposed upon the medical world. They have, it appears, not only assumed to themselves the privilege of naming our medicines, but also of changing those names every five or six years. One of my ablest and most diligent pupils (Mr. Moore) has taken the trouble of drawing up a table, showing the various names which have been successively bestowed on each substance since the days of Lavoisier. I have the table here before me, and I find that most chemical substances have, in the space of fifty years, undergone at least five changes. Of course, as the march of chemistry progresses with accelerated speed, we may give our nomenclators credit for an increased tendency to revolutionize the chemical vocabulary, and conclude that they will change them five times within the next fifty years. In 1890, how will a man be able to recognise a substance whose name has undergone ten mutations? I am anxious to dwell on this defect as being pregnant with perplexity and confusion. It would almost seem as if some enemy to our profession had invented the chemical nomenclature for the purpose of retarding the advance of practical medicine. Of what use will a practice of Physic, published in 1800, be to the reader who peruses it in 1900? We all know how easily the mind of man is deterred by difficulties; how few there are who will submit to the labour of becoming genealogists in chemical names.

Many and able men foresaw this difficulty from the beginning, and raised their voices against the adoption of names meant to convey a knowledge of the chemical composition of mineral and saline medicines. Bostock and Murray have both written ably on this subject, and I regret much that their advice has not been duly weighed and considered. In practice, many serious inconveniences arise from this vacillating state of chemical nomenclature. Every apothecary knows that mistakes occur from day to day, owing to the shifting character of chemical nomenclature, and I think it is time for us to bestir ourselves, and make a stand against the useless and dangerous innovations of the chemists. We should come forward boldly, and declare that we will not be made the slaves of names. Compare our last Pharmacopœia with its immediate or penultimate predecessor, and the difficulties a physician has to encounter will be obvious. Are we to be perpetually called on to learn new names? Must an artificial method of forgetting become even more necessary than a *memoria technica?* Must my prescriptions of 1818 be translated into a new language, if I wish to employ them now? It is time, then, to protest seriously against having our memories loaded with a polyglot vocabulary, and our ideas confused by a perpetual alteration of names. I do therefore assert boldly, that much benefit would accrue from reverting to the old system, and employing names which have no direct reference to the substances. I do not see any reason why we should not continue to call calomel, calomel; nor do I see any advantage in giving it any of the numerous modern appellations supposed to indicate its chemical constitution. I am glad to find that this view of the subject has the able support of Dr. Sigmond. He quotes Professor Brande as being of opinion that "it is very inconvenient to alter pharmaceutical terms according to the changes in chemical nomenclature; and as physicians in practice have not come to accord in this particular, I can see no objection to the term *calomel* for one substance, and *corrosive sublimate* for the other, pharmaceutically speaking. It is a subject of deep regret," adds Dr. Sigmond, "that the attempt should be made, because it never can be successful; for some chemists will call calomel *protochloride*, others *chloride*, and some denominate sublimate *perchloride*, others *deutochloride*, and others again, as does the Royal College of Physicians, *bichloride*." How remarkably corroborated is the truth of these remarks by the fact, that at present nearly all chemists agree in considering calomel a *subchloride*, and corrosive sublimate a *chloride* of mercury!

What is the use of a name? To designate a thing—to point out any substance, so that when we call for it we may get *it*, and nothing else. This is all that is necessary. When you tax a name beyond this, you exceed the limits of ordinary language, and demand too much. The old names for our medicines are not inferior, in this respect, to the modern ones imposed on us by chemists. Tartar emetic is a good and significant name, and yet I perceive it has been altered several times before, and again in the last edition of the London Pharmacopœia. Why is it that the preparation of bismuth used in pyrosis has been three times changed in my own memory? What alterations have not the carbonates of iron and of alkalies undergone? As for Fowler's solution, corrosive sublimate, Mindererus' spirit, and Æthiop's mineral (all good standard names), they are now nearly extinct, and have been superseded by a new generation likely to prove as unstable as their predecessors. Many other substances have undergone the same fate. Where will the revolution stop? Indeed we seem, at the present moment, as far removed as ever from the establishment of a stable system of chemical names. The progress of investigation discloses almost daily new views of the mutual relations between the elements constituting compound bodies; the atoms associated together are divided and subdivided into new groups, and, consequently, the symbolical representation of every compound assumes a new configuration, and is subdivided by brackets, altering their places with each successive advance of science. The labours of Bornsdorff and Hare already threaten the nomenclature of Berzelius, and the *chlorure platinosopotassique* of the latter, now considered as a compound of chloroplatinous acid and the chlorobase of potassium, must then be called chloroplatinite of potassium.

In a retrospect of the progress of chemistry for the years 1846—7, published by Mr. Sullivan in the number of the *Dublin Quarterly Journal of Medical Science* for February, 1848, at page 243 is the following paragraph:—"Thus, $NaO, SO_3 + 10Aq$, would be *natan-afinwasue;* $2NaO, HO. PO_5 + 24Aq$, would be *jenatan-alan-apun-weso;* $NH_4 O, Al_2 O_3, 4SO_3 + 24Aq$, one of the most complicated formulæ, would be *atolan-telmin-ojafin-weso*, a word which is certainly longer than ammonia-alum, but shorter than crystallized sulphate of ammonia and alumina, and even than the formula, which has eighteen syllables when read, while the new name has only ten." If such names be ever introduced into our Pharmacopœia, I fear we must get over some of the aborigines of the South Sea Islands to teach us how to pronounce them!

If chemical names are still to be formed with the view of expressing chemical composition, there is no end to the complication and length at which they must arrive. If they express composition, it is worse than useless were they to do so incompletely. A name whose structure designates the nature of the thing named must, in chemistry, to be serviceable, designate it with perfect accuracy. Professor Kane has analysed, in one of his very able papers, a crystalline substance obtained by boiling the white ammonia subnitrate of mercury with solution of ammonia. Suppose this substance to be introduced into the Pharmacopœia, how can it be named in conformity with the principle which attempts to make each name expressive of the composition of the matter named? Its composition is stated by Professor Kane to be—one atom of nitrate of the oxide of mercury, *plus* two atoms of oxide of mercury, *plus* one atom of amide of mercury, *plus* two atoms of the nitrate of the oxide of ammonium, *plus* two atoms of the oxide of hydrogen. Even if the ingenuity of chemists had surmounted the difficulty of inventing a name capable of expressing the nature, number, and mode of aggregation of the above elementary atoms, is it probable that a name, so gifted, would be of a length manageable by either the tongue or the memory? Is it certain that future experiments may not unfold new views concerning the arrangement of the constituent atoms, and thus nullify the old, by requiring the adoption of a new designation?

The following apposite remarks on this subject are extracted from a review of Dr. Gregory's Chemistry, in the *London Medical Gazette* for October 3, 1845. The reviewer, in noticing some of the new organic substances described, and the metamorphoses which they undergo, says, "Clever as this exposition is, we fear that it will be as unintelligible as Coptic or Sanscrit, not only to practitioners, but to the present race of students, who are apt to look very closely to what concerns them in their examinations. There is, however, this consolatory reflection, that the examiners would themselves have to go to school again before they attempted to ask questions upon one half of the subjects introduced into this volume on Organic Chemistry. Without intending any disrespect to the examiners of the University of London, or the Apothecaries' Society, we do not think that there is one among the whole body who could describe off-hand the symbolical differences between the *Oxalate* and *Oxamate of the Oxide of Methyle* (p. 397), the composition of *chloro-phrenisic acid* (p. 511), or the construction of cinnamic acid from cinnamyle! The candidates for the diploma are therefore safe for the present!

"We agree with the author that scientific chemistry has been too much neglected in this country; but it is questionable whether which produce a very marked effect without their elements taking a direct share in the changes which ensue; those bodies originate, as it were, in action, which is subsequently propagated from particle to particle; they are uniformly substances in a state of change, and appear to act on the blood as yest does on a solution of sugar. In this class appear miasms, contagions, and the similar sausage poison of Würtemberg; the latter is an excellent example. Sausages, made in a peculiar way, are much used in that country; when ill-prepared, they become poisonous, and their effects are invariably fatal: the patient gradually dries up into a sort of mummy, and after weeks or months of misery death closes the scene; but there is no poisonous *substance* to be detected in the sausage. It is, according to Liebig, in a peculiar state of fermentation, which is not checked by the action of the stomach, and which, unfortunately, is communicated to the blood; it never ceases until every part capable of solution has been destroyed, and death of course must follow. *Miasms* and *contagions* act on the very same principle, and the reason that all are not affected by them seems to be, that they require the presence of a peculiar compound in the blood, which enters into decomposition, and when the whole of this peculiar matter is destroyed, the disease disappears. If there be much such matter, the case is *severe;* if little, the case is *mild;* and apparently in many contagious diseases, *the peculiar decomposable matter, once destroyed, can never be renewed, so that these diseases occur but once.*"

Such is Professor Liebig's theory of poisoning and contagion —a theory which, though it comes to us recommended by the abilities of the first organic chemist of the age, and sanctioned by his anonymous but able reviewer in the *Quarterly,* can nevertheless be easily proved to rest on almost as many assumed as *proven* facts. Thus how can Liebig so positively assert that there is no poisonous substance in the fatal sausages? True it is that no chemist has yet insulated such a substance; but Liebig knows better than any one else, how profoundly concealed any particular animal principle may be, by being mixed with a great variety of other animal principles. Thus how long did sugar, in the blood of diabetic patients, elude the searches of chemists? and yet they were looking for a principle with whose chemical qualities they were already accurately acquainted.

How much more difficult of detection must the poisonous principle be, which exists in so compound a body as a Würtemberg sausage? Besides, what chemist was ever sure that he was actually analysing a poisonous sausage? Here a special difficulty lies, for hitherto there has been discovered no *a priori* method of distinguishing a poisonous from a wholesome sausage until both have been eaten; that is, too late for analysis. How long has the poisonous quality of ergot of rye been known? and yet the principle to which its effects are owing, though often sought, has been only lately insulated.

It is obvious, therefore, that Professor Liebig's main example of his new pathological explanation is not by any means *proven*, and consequently it is unnecessary to follow him into the regions of fancy where he has been enticed by a specious and seductive analogy. Pathology will cease to be a science when the study of facts gives place to such reveries as the above-cited passage contains—relative to miasms, contagions, mild cases, severe cases, diseases occurring but once in life, &c., &c., &c. And yet I am sorry to say that one of our most distinguished lecturers, Dr. Watson, has, in his published lectures on the Practice of Physic (volume 2, p. 667, 1st edition), fully adopted these opinions.

In order to give the reader some idea of what Dr. Watson considers to be "*distinct conceptions*," and "*lights supplied by a theory*," I beg leave to quote from the doctor's lecture the following paragraphs:—

"Moreover, the light supplied by this theory gives distinctness to our conceptions respecting certain deviations from the regular course and type of these diseases; which deviations are not uncommon.

"Thus the symptoms which precede and usher in the eruption are sometimes slow, halting, and irregular in their progress; appear, and then recede, and re-appear, so that we are in doubt what is about to happen, until at length the disease declares itself in its decided and authentic form.

"We may suppose this to depend upon some tardiness or interruption of the process whereby the virus is (to use the ancient term) concocted.

"Again, the series of combination of symptoms that mark the specific disease is sometimes, as I stated before, *incomplete*.

We have the eruption of measles without the catarrhal symptoms; the sore throat without the rash of scarlet fever. And experience has found that, where the malady is thus imperfectly developed, the protection it confers against its own recurrence is also incomplete. To explain this double failure, we may reasonably infer a corresponding defect in the series of changes which the poison tends to produce in the mass of the blood.

"Glandular enlargements and chronic abscesses are frequent *sequelæ* of these exanthematous disorders. They may be considered to represent the dregs of the reproduced virus, which has been imperfectly eliminated from the system by the usual channels."

Very few observations are called for by these surmises of Dr. Watson; and certainly the learned doctor is rather guarded in his expressions, thus admitting that though he has given his adhesion to Liebig's theory, yet he seems to view the deductions to which it leads with considerable distrust. Indeed it is difficult to rest satisfied with reasoning which not only assumes gratuitously a certain thing to be the cause of a certain effect, but considers it a corroboration of that assumption, that whereas the effect is irregular in its progress, *we may suppose* the cause is so likewise.

It is still a greater triumph of logic to infer that, because a disease is incomplete, we gain anything towards the establishment of the true nature of its cause, by saying that we may reasonably infer a corresponding defect exists in the cause itself. To me the whole line of argument appears delusive; and as to the last paragraph, concerning glandular enlargement and chronic abscesses, it seems that Dr. Watson's conclusion involves a contradiction, for he attributes to the virus itself, and that by virtue of its chemical action, the production of several exanthematous diseases, each specifically distinct, and indeed as different from each other as an acid from an alkali, while to the dregs of the reproduced virus he attributes sequelæ—those glandular enlargements and chronic abscesses which so frequently appear after small-pox, scarlatina, or the measles. According to this hypothesis, three different animal poisons, all acting chemically, produce at first three different diseases, and at last the same disease. With regard to this hypothesis, I may further remark, that when a brewer takes a certain quantity of sweet wort, puts it in a vessel, and adds a given portion of yest* to it, he knows that if he simultaneously fills in the same way fifty similar vessels, the process of fermentation will produce in each thirty times as much yest as was originally added to the wort. But when the virus of small-pox is introduced into the blood of fifty individuals, is a multiplication of the small-pox matter thus proportioned to the quantity of blood in each? It certainly is not; *a fact* conceded by the supporters of Liebig's hypothesis, but which they try to evade by saying that the particles of the blood which are susceptible of this particular decomposition and metamorphosis exist in different proportions in different individuals.

This method of ratiocination is as inconclusive as it is novel, and may be aptly termed, arguing not *in* but *outside* of a circle.

The following quotation, taken from the *Provincial Medical Journal*, contains a condensed but very accurate analysis of Liebig's theory of heat, and the pathological inferences which necessarily appear to flow from it:—

"The carbon and hydrogen of food, in being converted by oxygen into carbonic acid and water, must give out as much heat as if they were burned in the open air. The only difference is, that this heat is spread over unequal spaces of time; but the actual amount is always the same. The temperature of the human body is the same in the torrid as in the frigid zone. But as the body may be considered in the light of a heated vessel, which cools with an accelerated rapidity the colder the surrounding medium, it is obvious that the fuel necessary to retain its heat must vary in different climates. Thus, less heat is necessary in Palermo, where the temperature of the air is that of the human body, than in the polar regions, where it is about 90° lower. In the animal body, the food is the fuel; and, by a proper supply of oxygen, we obtain the food given out during its combustion in winter. When we take exercise in a cold atmosphere, we respire a greater amount of oxygen, which implies a more abundant supply of carbon in the food; and, by taking this food, we form the most efficient protection against the cold.

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* We are glad to find Dr. Watson adhering to the old spelling of this word. He spells it as De Foe spells it in his *Robinson Crusoe*; this authority is probably as good as any the writer in the *Quarterly Review* could bring forward in support of his yeast.

A starving man is soon frozen to death: *and every one knows that the animals of prey of the arctic regions are far more voracious than those of the torrid zone.*[*] Our clothing is merely an equivalent for food; and the more warmly we are clothed, the less food we require. Were we to go destitute of clothes like certain savage tribes—or if, in hunting or fishing, we were exposed to the same degree of cold as the Samoyedes—we could with ease consume 10 lbs. of flesh, and, perhaps, a dozen tallow candles into the bargain, as warmly clad travellers have related with astonishment of those people. Then could we take the same quantity of brandy or blubber of fish without bad effects, and learn to appreciate the delicacy of train oil.

"We thus perceive an explanation of the apparently anomalous habits of different nations. The maccaroni of the Italian, and the train oil of the Greenlander and the Russian, are not adventitious freaks of taste, but necessary articles fitted to administer to their comfort in the climates in which they have been born. The colder the region, the more combustible must the food be."

It is, I must confess, quite new to me that our clothing is merely an equivalent for food, and the more warmly we are clothed the less food we require. Take the well clad and warmly clothed country squire, and compare the quantity of food he devours with that which is consumed by his ragged labourers, and it may be asserted that the balance will be as much in favour of the squire's food as of his raiment. The voracious Samoyedes referred to, however barbarous in their manners, are an extraordinarily warmly clothed race, and the semi-putrid fat and blubber of whales agrees with the stomach of the Laplander as well in the heat of summer as in winter. In the arctic and cold regions of the earth man is driven by necessity to subsist on animal food, which is supplied to him by the unfrozen depths of the ocean, for in those inhospitable regions vegetable life is almost a stranger, and therefore it is that the Laplander, the Greenlander, and the Samoyede subsist almost exclusively on animal food. In the expeditions of Franklin, Parry, and Ross, our countrymen braved all the rigours of an arctic winter on the same food which they were in the habit of consuming in milder climates; and if it be true, as stated in the above passage, that in the animal body the food is the fuel, and, by a proper supply of food, we obtain the oxygen given out by its combustion in winter; if this be true, it is strange that there is no record of its being found necessary to give our sailors more food during the extreme cold than at other periods.

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[*] I cannot guess how *everybody* comes to know all this; for my own part, I think it may be maintained that a Bengal tiger, or Cape hyæna, requires, in proportion to its size, quite as abundant *rations* as any of the arctic carnivora; and as to the vultures of Hindostan and Persia, where on earth, in air, or in water, can be found such gluttons? Neither do I think that any one (not to say *everybody*) would be prudent in counting on the abstinence of a shark, even within the tropics! Although religious ordinances prevent the Hindoos from eating beef, yet both they and the Arabs occasionally devour mutton in astonishing quantities. Those who ride over the Pampas, in South America, at the rate of 100 miles a day, exposed to a burning sun, subsist entirely on boiled beef and water, without a particle of vegetable food of any kind, and yet they attain to an extraordinary *condition*, and capability of enduring violent and long continued exertion. Liebig's theory must be very ductile, if it can explain how it happens that an exclusively animal diet agrees with man quite as well at the equator as within the arctic circle.

Facts are wholly inconsistent with many of Liebig's allegations. All hunting tribes of mankind, whether in northern, temperate, or tropical regions, subsist chiefly on animal food. This is true of the North and South American Indians, and it is true of the Hottentots, and indeed our travellers relate prodigies of gluttony enacted by the latter; for when, after a long fast, they suddenly obtain abundance of game, they will sit up the whole night occupied in cooking and devouring steak after steak unaccompanied by a morsel of vegetable food, and at such times, so indefatigable are they in the business of eating, that the party which over night had tightened their famine girdles to the last hole, have enormously distended abdomens on the following morning,—this, too, in the heat of Africa, where certainly no additional fuel was required for supporting the animal temperature. If Liebig's theory be correct, that animal food is peculiarly adapted to cold climates, how comes it that the most voracious carnivorous animals abound in the hottest regions of the earth? The Bengal tiger, and the African lion, and the boa constrictor of South America, together with alligators and crocodiles of the Nile, the Ganges, and the Oronooko, all subsist solely upon animal food; and, on the other hand, among the whale tribe it is observable that they abound in every variety of oceanic temperature, where the appropriate animal food occurs, and the same observation applies to fishes in general. Take the antelope and the gazelle of Africa, which would shiver from cold during the warmth of an English summer, and compare them with the reindeer, that bears with impunity, and that for months together, a temperature far below zero, and how can we explain the difference by Liebig's theory, for they both subsist on vegetable food? Facts such as these are not merely irreconcilable with, but destructive of, that theory.

I would not be understood here as wishing to depreciate any department of human knowledge. Far be it from me. Besides, the attempt would be useless. But I am anxious that you should concentrate all your energies on the proper objects of medical pursuit, and devote the largest share of your attention to those requirements which will render you good practitioners. I have seen students led astray by false notions, wasting half of the time which should be spent in hospital and by the sick-bed, in wandering through the fields on botanical excursions, or working in the laboratory, engaged in the solution of some unimportant problem. Now this is not what will teach them to relieve suffering and cure disease. When I look round me, and behold so many young gentlemen entering upon an honourable and important profession, I feel that my responsibility is great. I consider you all as instruments of good or evil, and cannot help being conscious that I should be guilty of a great crime, did I not use every means in my power to render you able and efficient practitioners. The teacher of clinical medicine, gentlemen, occupies in every nation a post of heavy responsibility. But when he happens to preside over the medical education of those who resort to the wards of a metropolitan hospital—when the metropolis is a British one, and the hospital destined to send forth annually practitioners to every quarter of the globe—to North and South America, to New Holland, to the Cape of Good Hope, to the East and West Indies, and the countless isles which, in either hemisphere, are visited by the British flag, then indeed does that teacher become himself an instrument of good or evil to an extent which it is fearful to contemplate.

He who gives instruction to a clinical class in Berlin, Stockholm, Vienna, or Paris, has much to answer for, if he discharge not his duties with zeal and diligence. Yet if he fails to make his pupils good practitioners, their errors, however deplorable, are circumscribed within comparatively narrow bounds, and

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limited in a great degree to their own countrymen. But the British teacher sits in the centre of a circle far wider than Sweden or Prussia, Austria or France; his pupils are to be met with practising in every climate, exercising their art in almost every habitable region of the globe, and dispensing the blessings of health to all races of mankind:—to the hardy white settlers of Canada, the aboriginal red-skins of North America, the Negroes of Jamaica, the Hottentots and Caffres of Africa, and the countless tribes of Hindostan.

In truth, gentlemen, the British teacher of practical medicine exercises an influence without parallel in importance and extent, and his opportunities of benefiting or injuring his fellow-men are incalculably great. If he neglect his duty, if he teach erroneously, his negligence and his errors in practice are multiplied indefinitely, by means of those whom he ought to have better instructed; the scene of his guilt—for it deserves no better name—becomes fearfully enlarged, for there is no country so remote that it may not contribute victims to the incapacity of his pupils. But if, on the contrary, he works with zeal and diligence; if he labours conscientiously and perseveringly in performing the important task he has undertaken, a compensation awaits him to which scarcely any member of any profession can attain. Can any reward exceed in value the reflection that he has assisted, materially assisted, in imparting practical knowledge to multitudes of enterprising young men, who, year after year, leave our hospitals to engage in the sacred duties of the medical profession, throughout the world? Is it not a high privilege to be enabled to combat death and conquer disease, as it were by proxy, in so many different localities? Can man enjoy a purer, prouder, more gratifying reflection? When I hear that a favourite pupil who has acquired a solid stock of practical knowledge in this hospital, has settled in any particular town or district, I cannot help feeling, on the part of my colleagues and myself, that we have been the humble means of conferring a blessing on the people entrusted to his care; and I cannot refrain from congratulating myself upon holding a situation which multiplies a thousand-fold our efforts to be useful, and enables us to stretch forth our hands to heal men of all nations and languages. The hero and the despot may extend a sovereignty over distant regions—may exert an unlimited control over millions of vassals—may dispense honours and rewards, or inflict punishment and death: they may, like Alexander, grieve at the narrow limits of a conquered world, and sigh for other scenes of glory, but they cannot chase away pain; they cannot bid the burning thirst to cease, or give back repose to the sleepless; they cannot impart feeling or motion to the paralysed, or sight to the blind; and, above all, they cannot imitate that almost godlike function of the healing art, by which man is enabled to recall to his fellow-man reason long banished, and restore to society the hapless victim of insanity.

Gentlemen, the profession we have embraced is the noblest that can engage the mind of man,—when diligently cultivated and conscientiously practised; but it requires great and persevering industry to enable the student to master all the difficulties that beset his path. Feeling this strongly, I have trespassed perhaps too long on your attention; but I thought it my duty to lay before you, as fully as I could, those views which I deemed best calculated for your adoption in the acquirement of practical knowledge.

LECTURE III.

ON THE PROPER MODE OF STUDYING PHYSIOLOGY AND MORBID ANATOMY.

It is quite evident that a knowledge of the functions and structure of the body in health is essential to him who undertakes the treatment of disease, and hence physiology has always occupied the attention of physicians. Physiology, however, may be studied in very different ways, and with very different objects, and, until lately, all those who were engaged in the cultivation of this fascinating science, not contented with observing the state of the different parts and tissues during health, the nature and quality of the secretions, the mechanism and operation of the different organs, sought to ascend from a knowledge of effects to an investigation of causes, and after they had classified the more obvious phenomena of living bodies, endeavour to ascertain, if not the very principle of life, at least those motions and causes of motion which result immediately from the action of the living principle. Having thus, as they conceived, obtained a more accurate knowledge of the conditions of health, they proceeded to form general explanations of the causes of disease, and frame general rules for their removal. This method, apparently so philosophical, and possessing so many attractions from the generality and simplicity of its application, has more than any other circumstance contributed to retard the progress of medicine.

Gentlemen, this is not only an ancient, it is also a modern evil. We live among systems. It is true that the practice founded on the mechanical, mathematical, chemical and humoral physiologies, has been long since abandoned: but the destructive system of Brown has but lately quitted the stage, where its place is occupied on the Continent by those of Broussais and Rasori, and in Great Britain by the system which derives all diseases either from derangement of the digestive function, or from inflammation.

Physiology does not legitimately embrace the study of vital actions, but merely aims at ascertaining and arranging their effects. The important facts, which its study discloses, are perhaps infinite in number. As long as we confine ourselves to these, we advance at every step, and all is clear and intelligible; but the moment we attempt to inquire into the causes and modes of vital action, we begin to retrograde, and all becomes hypothesis and confusion. Thus, an examination of the organ of sight, discovers a wonderful and beautiful optical arrangement, calculated to form on the retina a picture of external objects, exact both in its colouring and outline. The physiologist, examining with attention the different parts of the eye, and the laws of their respective refractions, investigates the means by which distinct vision is secured at different distances; he compares the human eye and its appendages with that of animals which live in water, those which soar into the highest regions of the atmosphere, and those which burrow under ground. He considers the eye of the mole—feeble, but protected against injuries likely to be encountered in carrying on its subterraneous works; of the eagle—which, poised high in mid-air, selects its victim from the distant pasture; of the fly—whose microscopic organ, with a range of vision scarcely exceeding the limits of contact, distinguishes objects the most minute; and in all, he finds variations in the optical instruments at once curious and intelligible. But when he endeavours to advance further in his inquiry, and tries to explain how an image painted on the retina produces vision, whether by the means of undulations arising from the rays of light and propagated along the optic nerve to the brain, or whether because the retina is a nervous expansion, highly organised and framed, so as to feel the coloured image painted on it; he is at once arrested in his progress by the barrier which is everywhere interposed between physical and vital actions—between the mechanism of the organs of sense and the mode in which they produce ideas between body and mind.

But has he, therefore, gained no real knowledge applicable to practical purposes, or has his time been merely spent in a pleasing but useless study? By no means. Being acquainted with the mechanism and arrangement of the optical instrument, he is often enabled to remedy its accidental derangements. By means of a concave glass he corrects a too speedy, by a convex a too tardy, concentration of the rays of light. When the crystalline lens becomes opaque, his knowledge of its connexions, nature, and position enables him either to remove it altogether, displace it from the axis of vision, or to promote its absorption, and in order to effect the latter purpose, he mechanically irritates it, knowing by experience, that after such an irritation, the process of absorption commences, although he is quite ignorant of the connexion between mechanical irritation and this vital process. He who inquires into the physiology of the brain and spinal marrow can never discover the nature of nervous influence, or the manner in which pressure on these organs destroys, or irritation deranges, the motions of the voluntary muscles; and yet the entire treatment of cerebral or spinal diseases, whether spontaneous, or from the effects of injury, is grounded on a knowledge of this physical fact: without it, we could not estimate the value or effects of morbid changes in the brain or spinal marrow. On this reposes the rationale of the treatment of all convulsive, paralytic, and apoplectic affections.

Although we know not the manner in which the eighth pair of nerves superintends the respiratory process, although we understand not how the phrenic nerves influence the motion of the diaphragm, yet a knowledge of these facts led to a means of relief for spasmodic asthma, and to the recovery of persons apparently asphyxiated, by means of the galvanic stimulus passed along the course of these nerves. Knowing that some of the nerves, distributed to the face, are destined for sensation, while others serve for muscular motion, in cases of tic-douloureux we divide the sentient and not the motive nerves. In these, and a thousand other instances, physical physiology supplies us with information at once interesting and practical; it would be still easier to prove, as in the cases of Brown and Broussais, that vital physiology, by involving us in the discussion of subjects beyond the powers of our reason, never fails to entangle its votaries in a labyrinth, amidst whose mazes they move without progressing, and consume in idle speculations that time and labour they ought to spend in the acquisition of useful knowledge. But I trust the period has at length arrived when this error will be avoided; for, on the whole, it must be confessed that, in consequence of a wrong method of studying, and a misconception of the true objects of physiology, this science has in many instances retarded the progress of practical medicine.

Let us next consider the connexion of morbid anatomy with practical medicine. Many have mistaken the end and object of morbid anatomy, and there are not wanting some who even deny its utility, while others again, in their zeal for its improvement, have endeavoured to extend its limits so as to make it comprehend and embrace in the explanations it affords, all the phenomena of disease. It is not easy to determine which of these parties has most injured the cause of practical medicine. Morbid anatomy comprehends not merely decided and permanent structural alteration, but embraces, so far as they are capable of being detected, even temporary physical changes in internal organs. In order justly to estimate its importance, we should recollect that the first alteration in the texture of a part is not the cause but the consequence of disease, for in every healthy organ the texture is natural, and as every change of texture is produced in consequence of derangement in the vital action of the vascular system of the part, it is obvious that structural alteration must in the first instance be always produced by functional derangement. Thus the physical alterations which attend external inflammation, the tumefaction, the heat, the redness are not the causes but the consequences of disease. But in thus reducing them to the rank of symptoms, do we diminish their importance? Certainly not. For being immediately connected, as effects, *with* the primary cause, they prove the most useful of all symptoms, in enabling us to ascertain the seat and progress of diseased action. In this respect they possess a manifest advantage over the general or constitutional symptoms. Thus, in cases of spontaneous gangrene, phlegmonous inflammation, or erysipelas, what practitioner would be contented to draw his indications from the general symptoms, disregarding the appearance of the affected part? And yet this is exactly what those persons do, who refuse the aid of morbid anatomy in the treatment of internal disease.

In external diseases, most of the physical changes in the affected part can be at once recognised; their diagnosis is therefore comparatively easy, and their treatment well established. In internal diseases, the case is widely different, the physical alterations are here beyond the cognizance of our senses; and, in order to ascertain their nature and situation, we must carefully compare the morbid appearances of internal organs, as revealed to us by dissection, with the symptoms during life.

Although alteration of structure is in the first instance produced by a disease in the vital action of the part, yet this structural alteration may itself become a new cause of mischief. Thus the vascular system of the lungs, from some unknown cause, assumes such a change of action as produces a deposition into the pulmonary texture of various fluid and solid products, by which the entrance of the air into its vesicles is prevented, and the respiratory function, one of the most important of the body, is thus considerably deranged. Again, whatever be the original vital derangement which causes scirrhus of the pylorus, the obstruction thus formed is a secondary cause of new and important symptoms.

Another consideration, which enhances the value of morbid anatomy, arises from the fact, that when diseased action fixes itself in any part of the body, whether external or internal, and there gives rise to physical alterations, experience teaches us that the progress of the disease may be often arrested by removing its effects. Thus, to recur to the example of external inflammation, the redness, the swelling, the heat of the part, are but symptoms, and yet we find great benefit from the applications of remedies capable of diminishing them: hence we leech, and apply cold lotions, &c.

From all these considerations it is evident, that whenever disease is attended with either a temporary or a permanent alteration in the tissue of an internal organ, it will be of the greatest practical importance to ascertain the nature and extent of that alteration, and the progress of practical medicine will be exactly proportioned to the accuracy with which this can be accomplished. Thus, how much has the treatment of pectoral diseases been improved by the application of auscultation and percussion,—means which are only useful by enabling us to ascertain the physical alterations induced by the disease, or, in other words, the morbid anatomy of the affected organ. Without their aid, how trace the progress and follow the increase or diminution of pulmonary inflammation?—how demonstrate the existence of dropsical or pleuritic effusion within the chest?—how detect latent pneumonia?—how distinguish with certainty pleurodyne from pleurisy? I could prove the utter impossibility of distinguishing many cases of bronchitic from tubercular phthisis without their assistance. I might refer to chronic emphysema of the pulmonary tissue, a disease of great importance, but actually unknown before the time of Laennec, who first accurately described it in the dead body; indeed, before the application of percussion and auscultation, a perfect knowledge of this derangement of the pulmonary structure in the dead body would not have assisted our diagnosis, for how recognise it during life? I might bring forward dilatation of the bronchial tubes, another disease wholly unknown before Laennec's time, and which, before his discovery, could not be recognised by the common method of observation. I might enlarge on the great utility of attending to the changes which take place within the chest in measles and scarlet fever; but the benefit resulting from an accurate acquaintance with the morbid anatomy of the thoracic cavity is now so generally acknowledged, that I shall rather choose my illustrations from other classes of diseases.

Nosologists, until very lately, were agreed in attributing considerable frequency to those cases of apoplexy and paralysis which arise from serous effusion in the brain, or from a mere functional inaction or debility of the cerebral and nervous systems. This opinion was founded partly on speculative grounds, and partly on inadequate and imperfect post-mortem examinations, and in practical books, the symptoms supposed to announce sanguineous, serous, and nervous apoplexy, were dogmatically laid down. What was the consequence? Most disastrous, as I have had occasion to witness in some parts of the Continent, where the elderly practitioners still adhered to the practice founded on this false pathology. What can be more melancholy than to see time wasted or misemployed in the exhibition of diuretics, given to promote absorption of serum effused into the brain, or of strong exciting remedies, such as arnica, camphor, &c., to overcome the nervous debility, in cases where copious depletion by the lancet and purgatives were urgently necessary. I do not deny that in some rare cases serous effusion into the brain is the cause of death from apoplexy. I have seen such an event supervene in chronic dropsy, but there the termination was very sudden, and the previous history left no doubt as to the cause; but in the majority of the cases formerly treated as serous or nervous apoplexy, a more careful examination would have detected marks of vascular excitement or local inflammation, a subject I shall treat at large when on the pathology of the brain. A similar error in morbid anatomy led to a similarly erroneous practice in the treatment of hydrocephalus, and many cases of general and local dropsy. The effusion occupied the sole attention of pathologists; the marks of preceding vascular excitement or inflammation escaped their notice.

Time will not permit me to enlarge upon the light which morbid anatomy, rationally pursued, has shed upon diseases of the brain. It is sufficient to remark, that some of the most important modifications of inflammation in that organ have been only lately discovered, and it is only lately that a minute and extensive examination of the different changes the brain undergoes in disease, has begun to introduce a certain degree of regularity and precision into a department where all before was confusion and inaccuracy.

Examples of the utility of morbid anatomy might be brought forward without number:—the discovery of local inflammation being at times the cause of a disease in most of its symptoms resembling common ague; the use of the lancet in the cold stage of ague, a practice which may be advantageously resorted to in cases where each return of the fit is accompanied by a recurrence of inflammation in a vital organ, as the lungs or brain; the connexion between inflammation of the mucous membrane of the stomach, and some of those symptoms of fever formerly attributed to mere debility; the influence of cerebral inflammation and congestion, in producing the symptoms formerly vaguely denominated typhus; the low character which fever assumes when accompanied by pneumonia (and that, too, often latent); the symptoms which are produced by follicular ulceration of the intestines, which so frequently occurs in the course of fever; the diagnosis between the pain produced by neuralgia of the abdominal nerves, and that resulting from structural diseases of the intestinal canal; a more accurate knowledge of the state of the mucous membrane in the diarrhœa of phthisis, and in intestinal tympanitis; the numerous improvements in the treatment of diseases of the ear, which followed Itard's investigations concerning the morbid anatomy of that organ;—these and many other discoveries, all replete with practical advantages, are the results of the attention of our contemporaries to morbid anatomy. And, were I to appeal to the records of surgery, I might bring forward examples, if not more important, perhaps more evident and striking; for the invention and success of most capital operations depend on a perfect knowledge of the structural derangements, the removal or cure of which is attempted. Of this, examples suggest themselves on every side, but none is more striking than the one devised by Dupuytren for the cure of artificial anus, the most disgusting and loathsome malady to which human nature is subject, and one deemed altogether incurable, until that excellent surgeon, by a combination of profound pathological and physiological knowledge, succeeded in planning and executing an operation,. that was alone sufficient to immortalize his name.

The study of morbid anatomy, however, is attended with no ordinary difficulties, and, when imperfectly understood, is liable to lead to erroneous results, for it requires much candour, much patience, and that experience which can be only acquired by long continued practice, to enable us to judge concerning diseased appearances. The power of accurately discriminating in the dead body the traces of disease, cannot be suddenly acquired, and so numerous are the various errors to which superficial observers are liable, that much injury has thus resulted to medical science, diseased appearances being in some cases overlooked, and in others recorded where they did not exist. Those who are aware how often the congestion which frequently takes place immediately before or after death, in the pulmonary tissue, and in the mucous membranes of the lungs and alimentary canal, alters the physical properties of these parts, so as almost exactly to simulate the vestiges of inflammation, will understand how it happens that in investigations connected with the real or supposed diseases of these parts, facts have been marshalled against facts, and observations arranged against observations, until the path which promised simplicity and order, terminated in perplexity and confusion. Hence the doctrines of Broussais received so many corroborations, and appeared to rest upon numerous series of undoubted and well authenticated facts.

The morbid anatomist must of all things beware of seeing too much. He must avoid imposing on himself by everywhere seeing exactly what he expected to see, and above all things let him not always force himself to see something; for many diseases proceed to a fatal termination without having produced any evident morbid alteration.

When I come to treat of the pathology of the brain and nervous system, I shall have occasion to advert to errors which late authors have committed from too great an anxiety, on the one hand, to reduce to a certain and definite system the morbid appearances of the brain and spinal marrow, as connected with their diseases, and, on the other, to find, in every case where the cerebral or nervous functions had been diseased, lesions of structure to account for the symptoms. Thus, to cite one of numerous instances, I shall hereafter endeavour to prove that epilepsy and mania often occur [illegible] without the existence of any organic alteration; and, indeed, that organic lesions are not necessarily connected with these formidable diseases, is sufficiently proved by the [illegible] striking manner in which they cease. Thus a gentleman of great literary reputation was many years [illegible] before his death, which happened in his [illegible]. From the age of twenty-five to [illegible] and frequently returning fits of [illegible] for thirty years, the disease [illegible] and during the last [illegible] he had but a single fit. I shall [illegible]

[illegible] series [illegible] their way into English and Irish publications on the pathology of the brain, and which I shall be compelled to speak of hereafter.

Having made the preceding observations on the dangers which arise from an ill-directed application of the studies of physiology and morbid anatomy to the practice of medicine and surgery, I feel myself imperatively called on to present the other side of the question to your view, in exposing the still more dangerous doctrine advocated by those who depreciate the value of pathology and morbid anatomy, as only instructive after the death of the patient—and even then as not unfrequently calculated rather to mislead than to advance the interests of practical medicine.*

It must be conceded that he who is only a physiologist cannot hope to cure disease, and that the mere morbid anatomist will be often misled by post-mortem appearances—if he have not attentively watched the progress of symptoms, and the effects of medicines during life; for, unless this be done, he will, as I have already said, often mistake secondary for primary lesions, will confound effects with their causes, and will refer to certain alterations of structure that which had originated in a functional disorder—a morbid state of parts very different from that which is observed after death. But when, to an accurate knowledge of physiology and morbid anatomy is joined an extensive observation of the progress of symptoms and the effects of therapeutical agents, how much more certain and satisfactory will be our practical decisions, and how much more likely our efforts to be attended with success, than if we merely studied disease at the bedside of the patient. In the latter case indeed we might become expert nosologists, be accurately acquainted with certain groups of symptoms, and even not unfrequently adopt the proper method of treatment. These symptoms, considered together, we would call by a certain name, and hand down to posterity this new acquisition of medical knowledge, perhaps clothed in the garb of a dead language, and invested with the false dignity of a learned tongue. But what have we really thus effected for posterity?—Our followers read our definitions of disease with an acquiescing admiration, and, sure of the efficacy of the remedies we have recommended, they go forth with an overweening confidence in quest of the group of symptoms we have described, and when they have met with them they look upon their task as already half accomplished, and promise a successful termination of the disease.

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* The dangers above enumerated may be almost all avoided by institutions such as the Dublin Pathological Society, founded in 1838, and by means of which morbid specimens are exposed to an examination most likely to disclose their real nature.

"Tell me the name of the disease," was the motto of the nosologist, "and I will tell you the remedy;" but, gentlemen, I will engage to tell you the names of a hundred diseases, without your being able to name the proper method of treatment. I tell you a man has dropsy, his limbs are anasarcous, water is accumulated in the peritoneal cavity, his urine is scanty, and his thirst increased. Will you, from this very excellent nosological definition, venture to prescribe for this case of dropsy? For the sake of the suffering patient and your own conscience, prescribe not on such data. And yet I regret to be obliged to say, that such a method of proceeding is by no means rare, nay, it is even a matter of daily occurrence. But this case of dropsy will not yield. Some other boasted specific hydragogue or diuretic is had recourse to; still the patient grows worse and worse, and finally dies, but his friends are not discontented with the medical attendant, who excuses himself by asserting that he has successively resorted to every remedy which has been recommended in dropsy; and in truth if you look over the list of medicines exhibited in rapid succession, you will probably find that his excuse is not unsupported by facts. But, gentlemen, these cases in which everything has been tried are exactly those in which nothing has been tried, in which medicine has followed medicine, and each symptom of disease has indiscriminately been the object of attack, until death approaches with accelerated steps, and charitably closes a scene distressing to humanity, and disgraceful to the cause—I was going to say—of science; but who will venture to give so ennobling a name to this pseudo-practical knowledge, this worse than absolute ignorance?

Gentlemen, I am not combating phantoms; I do not, Quixote-like, contend with imaginary giants; no, gentlemen, what I have described exists, the picture I have drawn has many an original. But let us have done with this subject; let us turn to the gratifying considerations of the progress which practical medicine is making under its parent sciences,—physiology and morbid anatomy.

The reason of man is now more fully employed than at any former period; a vast store of mental power, a vast mass of mind is everywhere at work; what formerly was vainly attempted by the labour of a few, is now easily accomplished by the exertions of the many. The empire of reason, extending from the old to the new world, from Europe to our Antipodes, has encircled the earth: the sun never sets upon her dominions,—individuals must rest, but the collective intelligence of the species never sleeps; at the moment one nation, wearied by the toils of day, welcomes the shades of night, and lies down to seek repose, another arises to hail the light of morning, and, refreshed, speeds the noble work of science!

All inquirers commence, as it were, at the same point, as the labours of their predecessors are equally at the disposal of all, and consequently it is not surprising we should often find them arriving together at the same end; thence the number of simultaneous discoveries of the same fact now so common. It is not unusual to find the publications of France, Germany, Italy, and England announcing the same discovery, and each zealously claiming for their respective countrymen an honour which belongs equally to all. I am sorry to say that, with some splendid exceptions, this interesting and innocent controversy has been carried on by other countries, while Ireland has put in no claim for a share of the literary honours awarded to the efforts of industry or genius. But, gentlemen, this state of inaction, this state of mental torpor, is daily ceasing, and the time has passed away when we could not point out among our brethren any who had advanced the boundaries of the medical sciences, and thus promoted the interests of humanity.

Now we can enumerate many whose names form a catalogue the subject of congratulation for the present, of happy augury for the future; for cold must be the breast of him who will not hail with joy every symptom of our country's literary regeneration, —dead the feelings which are not elated at the boon conferred on our species by every advance made by those who devote themselves to the grand, the noble pursuit of relieving the suffering, of healing the diseased. But time bids me stop: I shall, therefore, conclude by observing that the attention lately devoted to the distinctions between real and pseudo-morbid appearances, the diligent cultivation of morbid anatomy by men not the slaves of preconceived opinions, the abandonment of all systems whose baseless fabric rests on the phantoms of vital physiology, the importance now justly attached to medical statistics, to the study of endemic and epidemic maladies, to the operation of morbid poisons: these, and various other circumstances, give us reason to hope that the progress of the human mind in investigating the means of preventing and curing diseases will not be less rapid than it has been in the other departments of knowledge. And thus it will be proved that if man has passions which impel him to the destruction of man, if he be the only animal who, despising his natural weapons for attack or defence, has devised new means of destruction,—he is also the only animal who has the desire or the power to relieve the sufferings of his fellow-creatures; the only animal in whom the co-existence of reason and benevolence attests a moral as well as an intellectual superiority.

LECTURE IV.

THE PULSE.

The posture of the body has a very considerable influence on the frequency of the pulse, even in healthy persons, and this influence being still more marked in disease, it has been long a matter of common observation, that the pulse is more frequent in the erect than in the horizontal posture. This subject, not having been investigated with the accuracy it merits, I have made it the object of numerous experiments, the results of which appear in some respects novel, and not devoid of practical utility. In healthy persons the pulse in the erect posture is more frequent than in the horizontal, by from six to fifteen beats in the minute. If the pulse is but sixty, the difference is generally not more than six or eight, and this difference increases with the frequency of the pulse at the time of the experiment; thus if it has been raised to 90 or 100 by moderate exercise, it is not unusual to find the difference twenty or thirty.

As the muscular exertion necessary to keep the body in the erect posture might be considered as the cause of this greater frequency, it became necessary to contrive means of placing the body in any desired posture, without the necessity of muscular exertion on the part of the subject of the experiment; this was effected, and it was found that when the posture was changed by means of such a contrivance, the difference between the frequency in the horizontal and the erect posture was not less than when muscular exertion was used.

I now anticipated, that if the body was placed with the head down and the feet up, a still further retardation of the pulse would be produced; it was, indeed, natural to conclude, from the preceding experiments, that posture alone was the cause of the retardation observed in the body when placed horizontally, and, consequently, that this effect would be augmented on still more depressing the head, and that the maximum of retardation would occur in the inverted position.

gated this subject, I have found a difference between the frequency of the pulse in the erect, sitting, and horizontal postures; *but in six cases of hypertrophy with dilatation of the heart, no such difference was perceptible, although all these patients, at the time of my making the experiment, were in a debilitated state,* which, it will just now appear, is that in which the changes induced by position are the most remarkable. In four of these cases the existence of hypertrophy with dilatation has been ascertained by post-mortem examination, and of the other two, a man and a woman, at present in the Meath Hospital, there can be no doubt of the state of the heart in one of them, while in the other the existence of hypertrophy is more than probable. For the sake of accuracy I shall give the precise results of the experiments I made before you on these six patients; where two numbers follow each other, they denote successive quarters of a minute, that being first which immediately followed the change of posture.

DOYLE,	*Monday,*	Pulse in Horizontal position,	72
		——— Sitting,	72
		——— Standing,	80
	Tuesday,	——— Horizontal,	72
		——— Sitting,	80, 72
		——— Standing,	80, 72
	Wednesday,	——— Horizontal,	72
		——— Sitting,	72
		——— Standing,	72
MALONE,		Pulse in Horizontal position,	60
		——— Sitting,	76, 60
		——— Standing,	76, 60

In both of these cases, although the pulse during the first quarter of a minute after the change of posture rose in frequency, yet in the next it fell to the previous standard; indeed, it may be remarked that the greatest frequency, *where muscular exertion has been used* to assume the sitting or erect posture, is observable in the first ten seconds which follow that exertion, both in health, and still more remarkably in disease; and consequently the first quarter, or even half of a minute, should be rejected where we wish to ascertain the permanent alteration thus produced.

In two other cases, *Gorman* and *Reilly*, in whom the hypertrophy and dilatation had attained to a great size, even this acceleration during the first few seconds was scarcely perceptible, and the pulse almost at once resumed its former standard. The same observation applies to the two patients at present (5th July) in the hospital: in the man the pulse is 76, both when he is lying or sitting; in the woman, in whom certainly extreme hypertrophy with dilatation exists, the pulse is constantly above 100, and the same in both postures. They have been both long ill, and are much debilitated by the effects of the disease, and of the remedies employed to mitigate its violence.

In these cases of diseased heart I have already remarked the hypertrophy and dilatation were very great, and in five of them certainly, and in the sixth probably, the left ventricle was involved in the disease; and I am inclined to think that this permanence of the pulse in all positions of the body will be only found to exist in such cases, and not in those where the hypertrophy and dilatation are less considerable, and consequently the diagnosis more obscure. This circumstance may, it is true, detract from the value of the observations *so far as regards diagnosis*, but certainly does not diminish its physiological interest. I may observe, too, that should future observations prove that hypertrophy of the heart is not always attended by this permanency of the pulse, and I believe it is not, yet its occurrence in so many cases of that affection is nevertheless an interesting fact. In pursuing this inquiry, it will be necessary to compare the effects of posture in hypertrophy with and without disease of the valves of the heart and aorta. It would be premature to inquire into the cause of this phenomenon, but it immediately suggests itself to the mind, that it depends on the increased strength and energy of the left ventricle when in a state of hypertrophy, and which, in a great measure, place its contractions, as it were, beyond the influence of these causes which, in other diseases, attended with debility, and even in many persons in health, enable a change of posture to produce so remarkable an alteration in the frequency of the pulse. I shall now give the results of a great number of observations, made both in hospital and in private practice, upon this effect of change of posture on the frequency of pulse in other diseases.

1st. That the greatest difference occurs in patients labouring under fever, or in a debilitated state in consequence of fever or any other cause. It may amount to 30, 40, or even 50, between the horizontal and erect postures.

2dly. That this difference decreases after the first quarter of an hour in most cases, but always remains considerable as long as the same position is observed.

3dly. That in persons not much debilitated the difference is much less than that stated above, and often does not amount to more than 10.

4thly. That when the patient lies down, the pulse rapidly falls to its former standard.

5thly. That in some the increase in frequency is greater between the horizontal and sitting posture than between the latter and the erect; while in others the contrary takes place, so that generally the frequency in the sitting posture may be taken as a *mean*.

6thly. In persons convalescent from fever or acute diseases, I find it is extremely useful to the physician to ascertain the comparative frequency of the pulse in the horizontal and in the erect position. The greater the difference, the greater is the debility of the patient, *and consequently the more guarded must his medical attendant be in allowing him to sit up for any length of time*, particularly if the pulse on his lying down does not resume its usual degree of frequency.

In the case of a young man named St. Leger, who was lately a patient at Sir Patrick Dun's Hospital, the variation of the pulse in different positions of the body was very remarkable. He was just recovering from fever, and exhibited a state of the pulse which is not unfrequently observed under similar circumstances. During his convalescence the pulse went on declining in frequency, until it sank to thirty-six in the minute. When I made him sit up in bed, his pulse began to rise rapidly, and, in the space of a minute, was at sixty-four. When he stood up, it became much quicker, *but grew so weak and indistinct that it could not be felt at the wrist.* On applying a stethoscope over the region of the heart, I found that its pulsations amounted to 112 in the minute. Here is a very remarkable difference of pulse depending entirely on change of position. With respect to the number of respirations in this young man, I found that when lying down they were only fourteen, but when he stood up they were thirty. This is a very curious fact, and one which I have not before observed.

In this case, the pulse was very little more than in the propor-

[illegible] compared with respiration, [illegible] We had another case at [illegible] the pulse was [illegible]4, and [illegible] a third case in which the [illegible] respiration was [illegible] I have [illegible] the pulse was [illegible] and the respira- [illegible]

[illegible] the pulse and respiration [illegible] and pulmonary [illegible] whose pulse [illegible] last twenty-four [illegible] the pulse [illegible] Would [illegible] present or past [illegible] For [illegible] I would rather have the [illegible] A [illegible] of the [illegible] lesion of the thoracic [illegible] powers of organic [illegible] and respiratory [illegible] that the case is of [illegible]

[illegible] accurate obser- [illegible] between the [illegible] of the system, and in [illegible] might be easily [illegible] and instructive [illegible] paragraph, [illegible] possesses atten- [illegible] opportunities for obser- [illegible] in the frequency of [illegible] I may here remark [illegible] the validity of the [illegible] this circum- [illegible] disease of the [illegible] considered as estab- [illegible] sudden change of [illegible] frequency of the pulse, [illegible] at least its left ventricle, is [illegible]

[illegible] prognostic sign of great value in many diseases. The following conclusions of much practical importance are, I think, especially deserving your careful attention:—

In fever, a dicrotous pulse, which is at the same time hard, is *a very bad* symptom, if it last more than twenty-four hours: when succeeded by epistaxis, and when it disappears after moderate epistaxis, it is not bad; it may in the same fever thus appear and disappear several times, but each time it becomes more serious. When, in fever, a hard dicrotous pulse lasts for many days without any tendency to hemorrhage, the case, in nine out of ten, ends fatally.

In hemoptysis, epistaxis, and internal inflammations, a very hard dicrotous pulse sometimes occurs, which resists all treatment, and portends a fatal issue; no matter how much the other symptoms may improve, so long as the pulse continues of this character, the patient is in imminent danger.

To return, however, to what I was before speaking of—the effects of posture on the pulse—authors who have written concerning the effects of digitalis on the organs of circulation, speak of the difference between the pulse, as observed in different positions, as an inexplicable anomaly, and seem quite ignorant that a similar phenomenon occurs in a less degree in health, and in an equal degree in many diseases. The fact appears to be, that *digitalis*, besides a great and debilitating influence on the whole constitution, and particularly the nervous system, possesses a *peculiar* power of diminishing the frequency of the pulse; *but it is no anomaly* that, in persons under its influence, debilitated and nervous as they always are, when it is exhibited in doses sufficient to retard the pulse, there should be a great difference between the frequency of the pulse as examined in the horizontal, the sitting, and the erect postures.

I need scarcely add, that I cannot advance even a plausible conjecture concerning the reason why a change of position should so affect the frequency of the pulse. It is singular enough, however, that Humboldt should have observed something similar in the hearts of frogs, cut out of the body, the great vessels being tied. In one of these experiments the heart being placed on a piece of glass horizontally, after twelve minutes its pulsations had sunk to twelve in a minute. It was now suspended perpendicularly, and after two minutes the number of pulsations rose to twenty.[*] Baer, in his work, *Uber Entwicklung geschichte der Thiere*, &c., has made the curious observation, that in hatching eggs artificially, the chick *in ovo* soon dies if the egg be so placed as to rest on either end. This circumstance, which he does not attempt to explain, suggests an obvious and beautiful explanation of the reason why eggs are not round but oval, as the latter shape effectually prevents them from assuming a position in the nest which would be fatal to the enclosed fœtus. Some ova, as for instance those of certain reptiles, are round; but I know of no bird whose eggs are not more or less oval. It would be interesting to investigate the cause of this phenomenon, as also to examine into the reasons of the remarkable difference which exists between the effects of position on the human fœtus *in utero*, and on the human adult. In the former the inverted or semi-inverted position of the body is the natural position; in the latter it is insupportable for any length of time.

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[*] *Annals of Medicine*, vol. iv. [illegible]

LECTURE V.

THE GENERAL LAWS OF INFLAMMATION. — MARSHALL HALL'S VIEWS.—THE CIRCULATION OF THE BLOOD.—INFLUENCE OF THE CAPILLARIES.

GENTLEMEN,—The general laws which govern inflammatory action, and the relation which the vascular system bears to that process, constitute a most important subject, which has engaged the attention of the ablest pathologists and practitioners in this country for the last half century. Since the date of the great John Hunter's celebrated work, which gave the first impulse to this investigation, many British and Continental writers have applied their talents to the illustration of the changes the vascular system undergoes during the progress of inflammation. Thomson, Hastings, W. Philip, James, Burns, and Marshall Hall have performed numerous and interesting experiments, which throw light on its phenomena; and we have gained much by the assiduity and research they have displayed, in endeavouring to illustrate a matter of such acknowledged difficulty. Still, these authors appear to have adopted some erroneous views, and to have misunderstood or overlooked some points of peculiar importance. I shall first direct your attention to the opinions of Dr. Marshall Hall, as explained in his lectures published in the *Lancet*. Dr. Hall, possessing extensive acquirements and high professional reputation, has cultivated the sciences of physiology and pathology with distinguished zeal, and made numerous experiments and microscopical observations, tending to illustrate the subject of inflammation; his opinions are, therefore, entitled to serious consideration.

Speaking of the inflammatory process, Dr. Hall observes—"I conclude that each cause of inflammation first induces such a physical effect upon the internal surface of the capillaries, as leads to the adherence of the globules of blood to it, and to their ultimate stagnation. This stagnation augments as the inflammation increases, and becomes more diffused, and seems to constitute the essential character of the disease." Here you perceive that he believes the first step to be the adherence of the globules of the blood to the internal surface of the capillaries; the consequence of which is, that the calibre of these vessels is considerably diminished, so that they become obstructed, and cause a stagnation of the blood, which Dr. Hall looks upon as the essential character of inflammation.

Further on he says—"I have never been able to detect any action in the capillaries themselves. It is, probably, by the partial obstruction to the circulation in the capillaries, that the minute arteries become enlarged." Now observe, according to this mode of explanation, the circulation being obstructed in the capillaries, in consequence of the adherence of the globules of blood to their sides, the arteries which supply them are propelling blood into obstructed vessels, and consequently become enlarged or dilated—and why? Dr. Hall says, "according to the well-known law, that muscular organs augment, with obstacles to their functions." Here I may, in the first place, observe, that Dr. Hall is not warranted in looking upon the minute arteries as muscular organs; but, waiving this point, how can the law alluded to explain the supposed increase in the capacity of the minute arteries? It might, indeed, explain the increase of thickness in their parietes; but is it not plain, that this very addition to the thickness of the arterial walls, so far from increasing, must diminish their calibre?

Again, he observes—"It is probably by the fact of stagnation that inflammation differs from blushing, eruptions, &c." Here, you perceive, he introduces the qualifying term, "probably." He continues—"It is generally asserted, that there is a series of vessels which only circulate the serum of the blood, and exclude the globules. This I believe to be mere hypothesis. Vessels which only admit of single globules will appear colourless. In inflammation, the minute arteries which only admit single globules at a time enlarge, and admit a greater number, and then the red colour becomes visible." He goes on then to say—"This enlargement of the blood-vessels is not confined to the minute arteries, for the larger vessels in the immediate vicinity of the inflamed part also become enlarged. * * * * This is owing to the obstruction of the true capillaries." And he illustrates this by instancing the application of a ligature to an arterial trunk, the consequence of which is, that the collateral arteries of the part become increased in size, in consequence of the obstruction. We shall see afterwards, how little this admits of being proved. He says—"It is not known how far this enlarged state of the arteries extends from the seat of the inflammation; but, in the case of an inflamed finger, the pulse at the wrist of the corresponding arm beats more strongly than it does on the opposite one."

Such are Dr. Marshall Hall's views of the causes of inflammation, and the part which the capillaries and minute arterial vessels play in that interesting process. You perceive, by the brief outline I have given, that he attributes all the phenomena to adherence of the blood-globules to the sides of the capillaries, the consequent obstruction of these vessels, and the enlargement of the minute arteries to which that obstruction gives rise. In this view of the case the vessels are regarded as passive, and are distended on purely mechanical principles; in fact, their enlargement is a mere dilatation.

Notwithstanding the respect I entertain for the learning, ability, and industry of Dr. Marshall Hall, I must say that I look upon his views as purely hypothetical, and am convinced that he has arrived at unsound conclusions with respect to the nature of inflammation. I shall not, however, take up your time by going over his positions *seriatim*, and showing their untenable character; but shall proceed at once to lay before you the opinions to which observation and reflection have led me, and which have been taught for many years in my lectures on the Institutes of Medicine. I shall not, like Dr. Marshall Hall, attempt to explain the nature of inflammation, or determine its proximate cause, but shall content myself with endeavouring to arrange its phenomena, and point out their order, and the share which the capillaries have in the inflammatory process. Before entering on this subject, it may be necessary to premise a few observations on the circulation in general.

The human body is composed of various parts, differing in their ultimate structure, chemical composition, and vital functions. There is a very remarkable difference between muscle and areolar tissue, and between the latter and nervous tissue. If we examine these parts more closely, we find them differing not only in their structural arrangements, but also in the ingredients or materials of which they are composed. In muscle we find a large quantity of fibrin or colouring matter; in cartilage, fibrous membrane, and tendinous substance, we find more or less of the *fibrous structure* of muscle, but we do not meet with *fibrin*, and there is not the slightest trace of colouring matter. The same blood furnishes materials for the growth and nutrition of all, and conveys the nutrient particles to red and white tissues alike; but the white parts require not red blood, and consequently receive none. Blood is a compound fluid, which contains, as it were, the raw material of all the tissues in a fluid state; it is, in fact, flesh in a state of fluidity, and destined to combine with and support the solid portions of the frame. It is conveyed by the arteries all over the body, supplying each tissue with its appropriate materials, and contributing to its growth, sustentation, and repair, in the amplest, and yet in the most economical manner. It does not enter the tissue of every organ in that state which has been termed arterial, and in which it appears as a fluid of a bright red colour. This is an error of which nature is never guilty. It would be absurd if all parts of the blood were carried to all the different tissues indiscriminately; and it would, moreover, be a great waste of vital and mechanical power. The chief bulk of the blood is made up of a transparent fluid or lymph, holding in solution various salts, besides albumen and fibrin. The red globules are immersed, but not dissolved, in this fluid; and it appears from the observations of Mayer, that in the minute vessels the red globules occupy the central part, surrounded by the transparent fluid. The colouring globules are necessary for the nutrition of muscular, mucous, and some other tissues; and are carried by the minute vessels wherever they are required. Every part of the blood is required in a muscle; fibrin and colouring matter for its essential fibre; albumen, fatty matter, &c., for its areolar tissue and adipose membrane. The white tissues, as I have already observed, receive no red blood, because they require none—this is quite certain. Serous membrane, for instance, contains neither fibrin nor colouring matter: at what point of the circulation does the separation of the albumen take place? Is it an act of nutritive secretion which separates it from the whole mass of arterial blood, or are only the serous portions of the blood carried to the white tissues? "Serous vessels," says

Müller, "that is, blood-vessels which are too minute to allow the passage of the red particles, and which are traversed, therefore, merely by the lymph of the blood, may possibly exist, but they have not been demonstrated."

It seems to me, however, that it is by no means necessary for blood-vessels to be too minute to allow the passage of red globules, in order to make these vessels the vehicles of lymph alone. The entrance of the globules into them will be determined by other circumstances than their size. Already, as the blood approaches the capillary system, the microscope detects a tendency to a separation between its lymph and colouring globules; and no doubt their complete separation is effected by vital agencies, independent of mere calibre. Hence we may explain the fact, that no red blood seems to circulate in serous membranes during health; but the moment inflammation sets in, the natural play of vital energies is deranged, and the red globules, finding their way into unwonted channels, vessels innumerable, before filled with a transparent lymph, and therefore not visible, start suddenly into view, in consequence of their now containing an opaque and coloured fluid.

According to Hall, Müller, and other physiologists, all minute vessels contain red particles, which, however, are believed to exert no influence on their colour, so long as these particles are only admitted singly, and not several at a time. But when inflammation comes on, according to Hall these vessels are enlarged in consequence of obstruction, and then, admitting a greater proportion of red globules, become visible. Now, gentlemen, observe how suddenly, when the conjunctiva connected with the sclerotic is irritated, numerous vessels appear filled with red blood. Here is no time for the adhesion of globules to the internal surfaces of the vessels—no time for the gradual enlargement of vessels previously too small for the admission of the red globules; no, the vessels existed there, but they contained no red globules; they admitted none, because their admission would have proved unnecessary or injurious. I do not deny the sudden enlargement of minute vessels; on the contrary, I believe in it most firmly, and am persuaded that the minute and capillary arterial branches which, in health, admit only lymph, may suddenly expand and increase in size. I do not, for reasons hereafter to be detailed, consider this expansion *tergo* alone, or the distance from the heart and size of the vessels,* but on the vital energy of the vessels themselves. Hear what Müller says on this subject: "Wedemeyer's description of the course of the blood in the anastomosing capillaries agrees perfectly with what I have observed. Sometimes, he says, the red particles flow rapidly from one current into another, as if by attraction. In other cases, the current which they join is very rapid, *but they are arrested, as it were, in the collateral current, and only from time to time find means of entering.* Sometimes a red particle is even thrown back out of the rapid current into a weaker stream, and is again repelled. I have also remarked that the same anastomosing branch between two currents sometimes receives the blood in one direction, and sometimes in the other, and that variations of pressure and position and motions of the animal are always the causes of these changes."

Such is Müller's testimony concerning the circulation of the capillaries, and it bears me out in the assertion, that a very great portion of blood (using that word in its most comprehensive sense, and meaning thereby *nutritive fluid*), is comparatively stagnant in the capillary system; but I must confess that I felt much astonished at Müller's assertion, that "all these variations in the capillary currents are, just as in currents of water on irrigated land, merely the results of mechanical causes."

Having made these preliminary observations, we are now better prepared to speak of the forces by means of which the circulation of blood is accomplished. Most authors, and with them Müller, have stated that the motion of the blood in the capillaries is wholly dependent on the heart's action. Now these vessels are mere simple membranous tubes, and there is no doubt that their membranous parietes must exert a strong power of endosmosis and exosmosis, as shown by Dr. Rogers in the *American Journal of Medical Science.* This power must necessarily have a great influence on the motions of the blood contained in the capillaries, causing a mutual interchange of contents between vessels in contact with each other, and between the vessels and surrounding parenchyma of the organs. Again, it has been proved by Dr. Draper, in the same journal, that in capillary tubes and organic pores a motion of the contents must result when the contained fluid possesses certain physical properties, from its mere contact with the internal surface of vessels so minute.

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* The blood's velocity in its progress from the heart is diminished chiefly by two physical causes, viz., increase of friction, and the increasing capacity (considered as a whole) of the vessels which contain it.

Here, then, are two sources of motive power quite independent of the heart's action, and which must necessarily influence, in a most important manner, the capillary circulation: but this is not all, for there resides in the small vessels connected with the capillaries, whether minute arteries or minute veins, *a vital sensibility* which enables them, by suddenly or gradually changing their calibre, to increase or diminish the quantity of fluid in any particular organ or tissue.

Facts in abundance may be brought forward in proof of this assertion. When a fatty or fleshy tumour arises on any part of the body, we have new vessels, as it were, created; and there is no reason to attribute their formation to anything like a dilating *vis a tergo*. But the formation of the vascular system in the fœtus affords the strongest proofs. Here the smaller and more minute parts are formed first, the development commencing with the capillaries and extending to the minute arteries and veins, and then to the larger trunks; until, at last, the heart is superadded, at first of an elementary, afterwards of a complicated structure.

The best account of the development of the vascular system in the fœtus is contained in Von Baer's work, published in 1837, in Konigsberg.* He says (Part II. p. 126), that there is no doubt that the blood is formed before the vessels. The formation of blood goes on in every part of the body, and, when formed, it is put in motion by the agency of some unknown cause which impels it in the proper direction, until it at length reaches the central formation of blood, around which is developed a tubular canal, afterwards to be further modified and changed into the heart. In truth the first motions of the blood are towards the heart, *and consequently the first vessels formed are the veins;* a fact in itself sufficient to disprove the hypothesis that this motive power which presides over the circulation resides exclusively in the ventricles of the heart. What do we find occurring in the case of pseudo-membranes resulting from pleuritic inflammation? Exactly what takes place in the development of the fœtus. A large quantity of lymph is effused, which at first has no vascular connexion whatever with the parietes of the chest. After some time, however, the effused lymph becomes organized, *and vessels begin to form in its substance*; these extend gradually, and join the vessels of the tissue with which the lymph lies in contact. Of this formation of vessels of effused lymph there can be no doubt; I have often examined it with admiration, and it is likewise attested by Andral. When a mass of lymph, effused into the pleural cavity, is about to organize itself, and become vascular, a vast number of red points make their appearance throughout the mass, and are connected with very minute streaks having a vascular distribution. In this lymph, then, red blood is manufactured, as in the fœtal body at an earlier period of development, and vessels are formed; and sanguineous circulation no doubt exists.

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* *Uber Entwicklung Geschichte der Thiere*, &c.

These facts, I say, bear strongly on the question before us, proving beyond a doubt that the vital properties of living matter are capable of forming vessels, and of rapidly increasing their size when formed. To account for the sudden increase in the size of vessels belonging to an inflamed part, we must look to this fact, and not rely solely on increased *vis a tergo* aided by obstruction.

Now the whole of Dr. Marshall Hall's explanation depends on these two causes—*vis a tergo* and obstruction. But I say that vessels may be formed, multiplied, and enlarged independently of these causes, and in consequence of an altered vital action of the parts in which the process occurs. Let me refer to the case of the impregnated uterus. In the unimpregnated state, the womb is a small organ, with vessels and nerves so small as scarcely to admit of being satisfactorily traced. What takes place after conception? It has now new and important functions to perform, and it becomes proportionally increased in magnitude and vital activity; its arteries and veins become elongated and enlarged; its walls become thickened, and its nerves increased in size. And yet we are told that this increase in the size of its vessels depends on obstruction. Where does the obstruction exist? What proof have we that there is any increased *vis a tergo*? Will any of these principles account for the augmented size of its nerves? Tiedemann has proved beyond contradiction that the nervous matter of the womb is augmented to a very remarkable degree during the impregnated state, and that minute nervous filaments, scarcely discoverable with the aid of a microscope, enlarge into bands visible to the naked eye. The same thing occurs with respect to the minute arteries and veins; from being but barely perceptible, they become large tortuous vessels, carrying an abundant supply of blood, and performing their functions with extraordinary activity. I do not pretend to offer any explanation of these facts; I merely place them before you, and show you the analogy which exists between the vascular and nervous development.

The vessels increase in size and capacity, so do the nerves; and the augmented size and capacity of both depend on the same unknown cause. The nerves are developed in the same order as the vessels, and, like the latter, they increase from the circumference to the centre. Nay, I am persuaded that, did our means of investigating the nerves possess the same advantages as those we enjoy in the examination of the vessels, we should find that, in inflamed parts, the nervous matter increases, in many cases, as rapidly and to as considerable an extent as the vascular.

So far, gentlemen, I have endeavoured to lay before you proofs of the independence of the capillary circulation, a fact which I have long since brought forward in my public lectures, and of which I have written somewhat in detail, in my views of Dr. Joerg's work on Atelektasis of New-born Infants. These views, I am happy to state, have been further confirmed by Dr. Houston, in his essays published in the tenth and twenty-fourth volumes of the *Dublin Journal*. In these essays, which I recommend to the attentive perusal of every student, Dr. Houston gives an account of an extraordinary case of twins born of a healthy young woman, between the seventh and eighth month of her pregnancy. One of the children was, to all outward appearance, perfect in every particular, and of the full growth of its age; the other, a female, and the subject of Dr. Houston's communication, was a monster, of somewhat smaller size than its companion. Both were alive at the time of delivery, but died almost immediately after. There was a separate cord, and a separate set of membranes, for each fœtus. The abnormal one had neither brain, heart, lungs, nor liver; the kidneys were of enormous size, nearly filling the abdomen, and extending to the apex of the cavity formed by the ribs. The umbilical vein, after quitting the cord, descended between the abdominal muscles and peritoneum as far as Poupart's ligament, and there opened into the external iliac vein, which became enlarged in size at this point. From this vein all the veins of the body were derived; large branches passed to the pelvis, thighs, and kidneys, and smaller ones to the intercostal spaces, and the tumour which constituted the head. These veins were devoid of valves, and terminated in the capillaries. From the latter, the arteries began by fine roots, and gradually coalescing, united into a sort of aorta on the forepart of the spine, which descending, divided into the iliac and hypogastric arteries in the usual way. No communication existed between the arteries and veins, except at their capillary terminations.

Such is the history of this very remarkable case, as given by Dr. Houston. I have not time at present to enter into his arguments; but I think he has satisfactorily proved, that in this instance the circulation was carried on without the aid of the heart of the other twin (as supposed by Sir Astley Cooper), or of the heart of the mother, and that it depended solely on the vital energy of the capillary and other vessels.

Another case of a monster without a heart, is related in the *American Journal of Medical Science*, for February, 1838, by Dr. Jackson, of Boston. This was likewise a twin; and there can be but little doubt that its circulation was quite independent of any assistance derived from the heart of its fellow.

I have already spoken of the dilatation of the arteries and veins of inflamed parts, as being produced by something very different from mere distention; and that it is not of a passive but an active nature. That the larger vessels actively dilate, can scarcely be doubted by any one who has observed the state of the temporal arteries in phrenitis or apoplexy; that the veins have a similar power may be observed on plunging the hands or feet into a hot medium, whether moist or dry. Blisters applied to the skin produce for the time increased size of the cutaneous veins; and sores on the leg may, when considerable and of long duration, give rise to a varicose state of the veins. When a grain of sand falls into the eye, how sudden is the redness—how numerous the vessels which now appear gorged with blood! This change takes place in a few seconds, and, in my opinion, can be most satisfactorily accounted for, by supposing that the capillaries and smaller vessels enjoy a wide range of size, if I may use the expression, and are capable of enlarging or diminishing their calibre, according to the exigencies of the case and the state of the circulation. That the large arteries and veins do so, is acknowledged by all, and is proved by arterial trunks contracting on their contents so as to maintain their proper tension, no matter how much blood is drawn from an animal. The larger veins are capable of a like contraction and expansion: can similar properties be denied to the smaller arteries, possessing, as they do, an elastic coat proportionally thicker? The vascular phenomena attending a blush ought to have taught physiologists how rapidly, how instantaneously, blood may be drawn to a particular part, and may again desert it; and that, under circumstances where the *vis a tergo* could not determine a flow of blood to the part in question, more than to any other in the body. Do we need microscopic examinations on the capillaries of recently killed animals to instruct us, when such phenomena offer themselves, as it were, for the very purpose of illustration? When the child breathes for the first time, the air admitted into the lungs gives new energy to their capillaries, and at once the great current of blood flows through the pulmonary arteries, deserting the ductus arteriosus. In a seven months' child the latter passage is still very large; and yet, when the child breathes, its being open effects very little, if anything, towards diminishing the flow of blood into the pulmonary arteries.

Here, again, we observe how arteries grow independently of mere pressure from within; for the pulmonary arteries and pulmonary veins are enlarging themselves long, long before they are called on to be channels for a quantity of blood at all proportioned to their calibres. John Hunter observed the enlargement of the arteries of an inflamed part, and his observations, and those of others, have brought to light a periodical and remarkable increase in the size of the vessels destined to promote the growth of the stag's horns. Are we, in this case, to explain that enlargement by obstruction, or by the *vis a tergo*? It is impossible to do so; and we must, then, look to the vessels of the part itself for a solution of the question. In such instances, as in the case of the pregnant uterus, these vessels are endowed with this power of growth and enlargement, quite independently of the general vascular system, or the action of its centre—the heart.

I am the more anxious to impress on you this view of the subject, as the hypothesis of obstruction has been adopted by many late writers, as explanatory of the local changes of circulation attending inflammation. Thus Dr. Williams, in his admirable lectures published in the *Medical Gazette* (No. 528), says, "We cannot, in the present state of pathological knowledge, doubt that the circulation through the inflamed vessels is, to a certain degree, obstructed; whilst, either as a consequence of this, or from some co-operating influence, the vessels leading to the part become dilated, and being thus more open than others to the pulse-wave of the heart, they become the seat of that throbbing hard pulse that has been mistaken for increased action of the vessels themselves."

Now, gentlemen, you observe here that Dr. Williams expresses himself doubtfully about the dilatation of the vessels being caused by obstruction, and he even speaks of some co-operating influence. We shall, therefore, content ourselves with having recited his opinion on this subject. I must observe, however, that the dilatation of the vessels, *however caused*, can on no principle account for their becoming the seat of throbbing, and a hard pulse: their being more open than others to the pulse-wave from the heart could, at the utmost, only place them in the situation of other arteries naturally of the size they have now attained to; but we do not find that such arteries throb, or have a hard pulse. Arteries do not throb, or become the seat of a hard pulse, in proportion to their size. That is not the fact; and, consequently, Dr. Williams's explanation cannot be admitted.

Dr. Weatherhead, who has arrived at very nearly the same view of the subject with myself, says, "The first effect of an excitant, or irritant, applied to any part of the body, is to attract the blood to the seat of irritation, and to quicken its current in the capillaries." So far we perfectly agree. Here Dr. Weatherhead estimates the vital energy of the vessels of the part at its true value, and does not call in the aid of an increased *vis a tergo* to account for an augmented determination of blood to any particular locality; but to what follows I cannot accede:—"If these effects be kept up beyond a certain period, or carried beyond a certain degree, the excitation continues to attract as much blood as before, while the power of the capillaries to forward it diminishes, by the exhaustion ensuing from their prolonged over-action." There seems but a weak analogy in support of the assertion, that increased vascular action must necessarily produce vascular exhaustion.

It may be objected to my view, that dilatation of an active nature cannot be conceded to the capillaries, whose coats are quite thin and membranous; but when the objects are so minute, it is quite impossible to determine the physical or vital powers of tissues; and we should recollect that what is deficient in degree may, in the case of capillaries, be made up by their number, which is immense in every part of the body. Still, so far as our observations do go, they seem to establish the property in question.*

Müller, whose opinion on all physiological questions is of the greatest weight, has adopted on this subject an hypothesis which appears to me to be quite untenable. It is observable that the first of the following paragraphs, which I quote from his work, proves, that when writing it, he felt conscious that the remarkable phenomena of *vital turgescence* are totally irreconcilable with the theory, which denies any permanent circulating power but that of the heart, and which asserts that "the motion of the blood in the capillaries is wholly dependent on the heart's action!" Let us hear what he says concerning *vital turgescence of the blood vessels*:—"Although it be denied that the circulation is in any way aided by an attraction between the blood and the capillaries, yet the existence of such an attraction or affinity may be admitted in the instance of the 'turgescence, turgor vitalis, or orgasm,' observed to take place in certain parts of the body, which are the seat of increased vital action, independently of the action of the heart. This condition of turgescence is very evident in plants: thus to the fruit-bud, which contains the impregnated ovum, there is, as Burdach remarks, an afflux of sap: ubi stimulus, ibi affluxus.

"*The mutual vital action, or affinity between the blood and the tissues of the body,* which is an essential part of the process of nutrition, is, under many circumstances, greatly increased; and gives rise to an accumulation of blood in the dilated vessels of the organ. It is seen, for example, in the genitals, during the state of sexual desire, in the uterus during pregnancy, in the stomach during digestion, and in the processes of the cranial bones on which the stag's antlers afterwards rest, at the time of the reproduction of these parts. The local accumulation of blood, with the dilatation of old, and the formation of new vessels, is, however, seen most frequently in the embryo, in which new organs are developed in succession by a process of this kind; while, on the other hand, other organs, such as the branchiæ of the salamander and frog, and the tail of the latter animal, become atrophied, and perish as soon as the vital affinity which existed between the blood and their tissues ceases to be exerted.

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* It is only this very year that physiologists have for the first time admitted that the middle coat of arteries, besides elastic tissue, is provided with muscular fibre. The discovery of this fact is due to the investigations of Henle, and has been confirmed by the electro-magnetic experiments of Ed. and E. H. Weber.—*Supplement to Müller's Physiology*, by Baly and Kirkes. 1848, p. 2.

"The phenomena of turgescence have been supposed to depend on an increased action or contraction in the arteries. But arteries present no periodic contractions of a muscular nature; and a persistent contraction of the arteries, unless it were progressive or vermicular, or aided by valves arranged in a determinate direction, would be quite inadequate to produce a state of turgescence in any part.

"To explain the state of orgasm of the uterus during pregnancy, and of the bony processes which bear the antlers of the stag, we must presuppose the existence of an increased affinity between the blood and the tissue of the organ. This condition may be excited very suddenly, in the instantaneous injection of the cheeks with blood in the act of blushing, and of the whole head under the influence of violent passions, in both of which instances the local phenomena are evidently induced by nervous influence. The active congestion of certain organs—of the brain, for example—while they are in a state of excitement, is a similar phenomenon.

"If the organ which is susceptible of the increased affinity between the blood and the tissue is, at the same time, capable of considerable distention, tumefaction and erection take place."*

It will, I believe, be readily acknowledged that Müller's explanation is, after all, a mere hypothesis. Is this affinity between the blood and the tissues of the body chemical? or is it a mutual vital action? If the latter, then the vessels, *they being the only tissues in contact with the blood*, are active, contrary to his previous hypothesis. As to the chemical explanation of a *blush*, it surely does not merit examination.

* *Müller's Physiology.* Translated by W. Baly, M.D. 2nd ed., vol. i. p. 238.

The facts referred to by Müller in the above passage all tend to corroborate the view I have adopted, and show that local changes of nutrition, vascularity, and circulation may be quite independent of the heart's action.

We must next turn our attention to the increase in size of some of the larger arteries.

"Apply a ligature," says Dr. Hall, "to the principal artery of a limb; the circulation is then carried on by the collateral branches, which become enlarged for this very purpose, and in consequence of the obstruction." Now let us study the phenomena a little more accurately, and we shall soon see how erroneous is this explanation.

In the first place, what are the physical results produced by tying one of the large arteries of a limb? The *vis a tergo*, or propelling power of the heart, continues just as before; the quantity of fluid or blood within the whole system of arterial tubes is unchanged, while the forces to be overcome by the circulating power remain also the same. In fact, all the general physical conditions are unaltered after the ligature has been applied, except that a portion of the blood can no longer enter the tied artery.

Let us now investigate what effects this non-entrance of a certain portion of the blood into its accustomed channel is likely to produce on the rest of the arterial system. When the principal artery of a limb is tied, the blood circulating in the remaining arteries of the body and the other arteries of that limb is pressed more strongly against the arterial parietes. But as the distending force resulting from this increased pressure is not confined to any particular artery of the body, but affects all, more or less, it is obvious that a power so extensively distributed and subdivided can exert but little distending influence on any individual artery, or, in other words, can tend but little to dilate any of the arterial tubes. Now it is obvious, from the laws of hydrostatics, that this increased pressure will be more exerted, *in proportion*, on the main collateral arteries of the limb than on the smaller; it will, in truth, be scarcely sensible in the latter, and yet these are the very arteries which enlarge first after the operation for aneurism. The increase in the size of the arteries commences, not where it ought to commence, if it depended merely on dilatation from increased pressure, viz., in the larger arteries and in

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the collateral branches close to the ligature, but it commences in the smaller and more distant arterial ramifications. In addition to the fact that a proportionally less pressure is thrown on the smaller arteries, we must recollect that the latter have parietes much thicker in proportion to their calibre than the larger branches. This is another material objection to Dr. Hall's explanation of their increase in size.

What are the phenomena observed after applying a ligature to an artery of large size, where a sufficient collateral circulation may be supplied? First, the sudden diminution of circulation in the parts below the ligature gives rise to coldness and paleness of the limb; but in a few hours the circulation gradually returns, the thermometrical temperature of the limb rises, and the activity of the capillary system is greater than in the natural condition of the limb. This excitement continues for some time, and then diminishes to the ordinary standard of health. In eight, twelve, or twenty-four hours, after the application of a ligature to the main artery of a limb, we find the skin of the parts below the ligature pale and cool, but in a few hours afterwards its temperature rises, and it exhibits an evidently increased arterial action. Now it is difficult to conceive that the main collateral branches have been dilated in so short a space of time.

The mode in which the phenomena witnessed in this instance are best explained, seems to me to be the following. When a large portion of the blood destined for the supply of a limb is cut off, all the tissues of a part so deprived receive a shock: the muscles, nerves, capillary vessels—in fact, the vital functions of the whole—are more or less affected. After some time, however, the vital depression is followed by reaction, and this commences in the smaller arteries and capillary system, its commencement being marked by uneasy sensations, increase of temperature, and arterial throbbing. The initiative of the restoration of the circulation belongs to the extreme vessels, which take on an increased action, and this is gradually extended to larger arteries. These gradually augment in power, become enlarged and distended, and at length the circulation of the affected limb is restored to a state of efficacy, equal, if not identical with its pristine condition. Now, you are told that the increased activity of the capillary vessels in this instance is referred to the *vis a tergo* operating through the anastomosing branches. This is a false assumption. In the first place, the influence of the heart's action, when the blood passes through small anastomosing branches, and by circuitous courses, must be less powerful than before the operation, when the main channels remained pervious. The *vis a tergo* is therefore lessened, and yet the capillary distention is greater than before the operation, or in the sound limb.

In the next place, this argument is of more force, when it is considered that the enlargement commences in the smaller, and gradually extends to the larger vessels; and also, that in some cases the branches from the main artery, through which the blood must pass, do not become perceptibly enlarged. Thus Mr. Hodgson, in his work on diseases of arteries, says,—"The dilatation takes place principally in minute ramifications. The trunks, and the mouths of the vessels going off above the place of obstruction in the main artery, in several preparations, did not appear larger than in their natural state, and in a few instances only a slight dilatation was perceptible." From these facts it is obvious that the vessels least under the influence of the heart are the most dilated. But the most decisive proof is the return of the capillaries and minute arteries to their ordinary size, at the time when, the larger branches being dilated, the *vis a tergo* should be more operative. Hence it would appear that the power of distention resides in the arteries themselves, the irritation commencing in the capillaries, and being sympathetically propagated to the larger vessels. This is further confirmed by the fact, that, if an inflamed part, or a vascular tumour, be supplied by several arteries, and one of them be divided, the others will enlarge.

It is scarcely necessary for me to direct your attention here to the phenomena which occur in the erectile tissues, as the corpora cavernosa, &c. It cannot surely be maintained that the sudden increase in the afflux of blood to these parts is owing to any *vis a tergo*, or momentary augmentation of the propelling power of the heart. No; tissues of this kind enjoy the power of attracting to themselves an increased quantity of blood, in virtue of the vital power resident in them, and not from any peculiar exertion of extraneous forces. In fact, the capillary vessels enjoy the property of actively dilating, and drawing the blood into them, and this appears to be one of the principal causes of the circulation. Of this there is abundant proof. It has bee observed in vivisections, that after the heart has ceased to ac the capillary vessels remain distended, and appear to carry c their functions as long as any blood is supplied to them from th arteries. It has been also remarked, that the larger arteri branches become first empty, then the smaller, and finally th capillaries. Dr. Philip states, that he has observed the circula tion of the mesentery to continue for several minutes after th heart had been excised. This is the true explanation of th fact, that the arteries are so frequently found quite empty afte death.

One of the strongest proofs we have of the power which th capillaries possess of drawing blood to themselves, is derive from the phenomena observed in vascular tumours. If scratche or slightly wounded, these tumours frequently bleed to a alarming extent; while the division of the arteries which lea to them, and the removal of the whole mass, is attended with comparatively small loss of blood. This is further exemplifie in the familiar operation of opening the temporal artery. If th artery be only partially divided, and its connexion with the capi laries still to a certain degree maintained, it bleeds copiously but if it be cut across, and the connexion wholly destroyed, ceases to bleed altogether. Professor Smith, of Philadelphia, amputated a leg below the knee, for dry gangrene of the foot an ankle. The great arteries were found wholly altered in thei structure, being, as it were, converted into tubes of bon Although pressure was completely removed from the femora artery, and no means whatever were used to suppress th hemorrhage, the quantity of blood lost did not amount to hal a table-spoonful. At the same time the action of the heart wa vigorous, and the pulse at the wrist of the ordinary strengt and fulness. Now in this case some blood must have bee passing through the tibial arteries before the operation, fo there was some circulation in the leg down as far as the ankl and the collateral arteries, or anastomosing branches, were no enlarged.

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* This fact is mentioned in a monograph which I received from America many yea ago. Unluckily I have mislaid it, and cannot call to mind the author's name. H advocated views similar to those I have here attempted to establish, and to him I a indebted for the argument derived from the placental circulation.

If we refer to the phenomena of wounds which engage arteries, we shall find, as I have already stated, when alluding to the operation of opening the temporal artery, that the wounded artery of an injured limb bleeds much more than the same artery of an amputated one. Hence it is that branches which would pour out a large quantity of blood, if merely wounded, sometimes do not require a ligature at all, although their divided orifices open on the surface of a stump. Another instance in which the attracting power of the capillaries may be seen, is in cases where portions of an amputated finger have again united, of which we have several examples. In this case the fluids effused by the upper cut surface are absorbed and circulated by the vessels of the amputated tip.

But one of the most remarkable proofs of the position I have laid down is derived from the circulation of the blood in the placenta. In this instance the impetus which the blood possesses in the umbilical arteries has been attributed to the *vis a tergo* derived from the heart of the fœtus. But after the detachment of the placenta, after the birth of the fœtus, the pulsation in the cord ceases; first at the placenta, and then at the umbilicus of the infant. After this period a section of the cord is not followed by anything like the amount of hemorrhage which might be expected from the division of vessels of such diameter, and in many instances there is no loss of blood whatever. Now why does the flow of blood cease in the umbilical arteries? The *vis a tergo* is as powerful after birth as before, and operates on blood in tubes free from obstruction. It cannot be attributed to cold, for the circulation continues in every part of the infant; nor to exhaustion, for the fœtus loses no blood, and its circulation is now independent of the mother. Neither is it owing to the action of the lungs, which are said to divert the blood from the placenta, for although a greater portion of blood is carried to the lungs, after than before birth, yet this would not account for the total cessation of the circulation in vessels so large as the umbilical arteries. The explanation, therefore, commonly given is not capable of being proved. From the facts which I have brought forward, it would appear that the organic vital actions of the placenta depend upon its own life, and that when this body is detached from the uterus, it of course dies, and the functions of its capillary system cease. The suction-power of the small vessels then continues no longer to assist the *vis a tergo* in carrying blood through the umbilical arteries, and the circulation declines, first at the placenta, and finally in the umbilical arteries, at their junction with the abdomen of the fœtus.

LECTURE VI.

INFLAMMATION.—THE CAPILLARY CIRCULATION.

GENTLEMEN,—I have now laid before you at some length the arguments derived from experiments and pathology in favour of the capillaries having a direct influence on the circulation. Those which may be borrowed from comparative anatomy are still stronger. If we look to the vegetable kingdom, we shall find that the force with which the sap—the blood of plants—circulates in their vessels is very great. Hales and Dutrochet have proved this by direct experiment. If a vine be cut down in spring to the distance of three feet from the ground, it throws out sap with such a force as to raise twenty-one feet of water. In other experiments this power was found capable of raising thirty-two and a half inches of mercury, or thirty-five feet five inches and three-quarters of water; and thirty-eight inches of mercury, or forty-three feet three inches and one-third of water. To effect this prodigious circulation, as it takes place in plants, the force must be very great, for we know that it is capable of raising from the ground a large quantity of water, combined with nutritious principles, to the top of the loftiest palm or forest tree—in fact to an altitude of one hundred and fifty feet.

Now in what organs does this power reside? There is no central organ in plants, nothing like a heart—nothing like large arterial tubes. How, then, is the ascent of the nutritious fluid accomplished? Let us study the phenomena for a moment, and we shall find sufficient evidence to satisfy ourselves that *the fluid circulating in each part of the tree is brought to it chiefly by the action of the vessel of the part itself.* I do not mean to deny the great power which the spongioles of the roots, acting as capillary systems, exert in *driving* the fluids they absorb through the tubular vessels of trees; this power is no doubt aided by the buds and leaves, whose capillaries, when acted on by a proper temperature, discharge their vital functions with activity, and are capable of drawing the sap to the extremities of the branches. Thus in the case of a vine observed by Richerand, one branch of which had crept into a smith's shop, this branch remained in leaf, or rather threw out fresh leaves in winter, while all the other branches continued quite bare. Again, cut off a branch of a living plant and place it in water, how actively does it absorb the water, and endeavour to prolong its existence. In winter this attraction of the ultimate ramifications of plants ceases, but returns again with the genial warmth of spring, when the buds begin to expand.

Phenomena analogous to these are also observed in many animals. There are numerous tribes of animals possessing an active circulation, which have no heart whatever. Thus the Medusæ and Echinodermata, which must enjoy an active circulation, as is proved by their rapid growth, have no heart. In the Holothurio tubulosa, Cuvier has traced vessels going to the organs of respiration (pulmonary arteries), and vessels coming from the same (pulmonary veins), as also a system of arterial and venous tubes destined to carry on the general circulation, but no heart. There are numberless examples of this arrangement to be found in the animal kingdom. In fact, a great deal of the motion observed in the fluids of the human body is effected by other means besides the heart, and these means are the powers possessed by the capillary vessels and membranous tissues of the body, which, by virtue of an unknown law, aid materially in the circulation.

You perceive, then, gentlemen, that my views are quite opposed to those who assert, that in inflammation the enlargement of the capillaries is passive. Dr. Hastings and Dr. Philip allow that the capillaries dilate during inflammation, but they attribute this effect to debility. This, however, is a mere assumption. The phrases, passive and debilitated, put one in mind of another hackneyed expression founded alike on fallacy, namely, indolent ulcers. Now there is nothing more active than what is termed an indolent ulcer. It manufactures more secretion, uses more blood, and produces more pain than any equivalent portion of the same tissue throughout the body, and yet it is termed indolent! It is so with regard to the capillaries. It is said that in inflammation the capillary vessels are obstructed, and their force weakened. What is the real fact? Take an instance of conjunctivitis. What do you observe here? The affected membrane is swollen, its nervous sensibility exalted, its thermometrical temperature increased, its secretion augmented. Are any of these symptoms of debility? I think they can hardly be looked upon as such. The increase of pain, heat, and fluid secretions, the augmentation in size, all the phenomena, in fact, are opposed to the theory of debility. There is no passive dilatation or weakness; the capillaries enlarge and dilate from increased, and not from diminished action; red blood finds its way into vessels which before received only white; and unusual secretions occur in the affected parts. *The capillaries have the initiative; with them commences the enlargement, which afterwards extends to the smaller arteries, and from these to the larger branches.*

Under ordinary circumstances, the capillary circulation continues some time after the heart has ceased to beat, for the capillaries belong to that class of tissues which possess an inferior degree of vitality; and it has been shown by Bichat that such tissues survive those of a higher degree of organisation. Hence, the capillaries continue to act for some time after the heart has ceased to beat; and as it is a law that the capillaries of the lungs will not transmit non-arterialized blood, the systemic veins become gradually distended, while the systemic arteries are emptied, so that, after death, we seldom find any blood in the latter.

A very curious case, published by Dr. Houston, supports very strongly the views which I have now put forward. In this case the circulation had ceased in one of the lower extremities. The foot, and afterwards the leg, were attacked with dry gangrene, of which the patient died. No obstruction was found in the vessels after death, and the ordinary injection passed readily into all the arterial ramifications. The arteries were all pervious, and apparently natural in their texture. Now, if the circulation of the limb had depended on the arteries alone, it would not have ceased so completely.

Some time ago I attended, with Mr. Cusack, a patient from the North of Ireland, a young lady of rather delicate constitution, who was attacked at a certain hour every day, in a very singular way. The circulation in one of her legs seemed almost to cease, and the limb became remarkably pale and cold. This state of the limb would last for ten or twelve hours, and then an alteration took place; the leg became hot and painful, and its temperature became so disagreeable to the patient that she was obliged to keep the leg outside the bed clothes, and have it constantly wetted with cold water and vinegar. During all this time the action of the heart was natural, and the circulation of the rest of the body unaltered. Here we have a certain portion of a limb at one period of the day quite cold and pale, and at another extremely hot and painful. How can this be said to depend on any *vis a tergo!* The true explanation of the matter is, that it depends on a periodic affection of the nerves, capillaries, and smaller arteries of the part.

Before I conclude this interesting subject, I think it well to lay before you the views of some celebrated physiologists, which coincide with my own, and are strongly corroborative of the doctrines which I have for many years advocated. I shall first quote the opinions of Dr. Carpenter, the most modern and one of the most distinguished of our British physiologists, from the third edition of his Treatise on Physiology (p. 568): their importance is sufficient apology for quoting them in this place.

"We now come to the last head of the inquiry into the powers which convey the blood through the capillary system —that, namely, which concerns the agencies existing in the capillaries themselves. Many discussions on this subject may be found in physiological writings, and it has so immediate a bearing on one of the most important questions in pathology —the nature of inflammation—that it deserves the fullest attention. The chief question in debate is the degree in which the capillary circulation is influenced by any other agency than the contractile power of the heart and arterial system; some physiologists maintaining that this alone is sufficient to account for all the phenomena of the capillary circulation; and others asserting that it is necessary to admit some supplementary force, which may be exerted either to assist, retard, or regulate the flow of blood from the arteries into the veins. We shall first consider what evidence there is of the existence of any such force; and, when led to an affirmative conclusion, we shall examine into its nature. No physiological fact is more clearly proved than the existence, in the lower classes of animals, as well as in plants, of some power independent of a *vis a tergo*, by which the circulating fluid is caused to move through their vessels. This power seems to originate in themselves, and to be closely connected with the state of the nutritive and secreting processes, since anything which stimulates these to increased energy accelerates the circulation, whilst any check to them occasions a corresponding stagnation. It may be convenient to designate this motor force by the name of *capillary power*, it being clearly understood, however, that no mechanical propulsion is thence implied. On ascending the animal scale, we find the power which, in the lower organisms, is diffused through the whole system, gradually concentrated in a single part, a new force, that of the heart, being brought into operation, and the circulation placed, in a greater or less degree, under its control. Still there is evidence that the movement of the blood through the capillaries is not entirely due to this, since it may continue after the cessation of the heart's action, may itself cease in particular organs when the heart is still acting vigorously, and is constantly being affected in amount and rapidity by causes originating in the part itself, and in no way affecting the heart. The chief proofs of these statements will now be adverted to.

"When the flow of blood through the capillaries of a transparent part, such as the web of a frog's foot, is observed with the microscope, it appears at first to take place with great evenness and regularity. But on watching the movement for some time, various changes may be observed, which cannot be attributed to the heart's influence, and which show that a certain regulating or distributive power exists in the walls of the capillaries, or in the tissues which they traverse. Some of these changes, involving variations in the size of the capillary tubes, have been already referred to; others, however, are manifested in great and sudden alterations in the velocity of the current, which cause a marked difference in the rates of the movement of the blood through the several parts of the area under observation. Sometimes this variation extends even to the entire reversion, for a time, of the direction of the movement, in certain of the transverse or communicating branches, the flow always taking place, of course, from the stronger towards the weaker current. Not unfrequently an entire stagnation of the current in some particular tube precedes this reversion of its direction. Irregularities of this kind, however, are never frequent when the heart's action is partially interrupted; as it usually is by the pressure to which the animal must be subjected in order to allow microscopic observations to be made on its circulation. Under such circumstances, the varieties in the capillary circulation, induced by causes purely local, become very conspicuous, for when the whole current has nearly stagnated, and a fresh impulse from the heart renews it, the movement is not by any means uniform (as it might have been expected to be), through the whole plexus supplied by one arterial trunk, but is much greater in some of the tubes than it is in others; the variations being in no degree connected with their size, and being very different at short intervals.

"The movement of the blood in the capillaries of cold-blooded animals, after complete excision of the heart, has been repeatedly witnessed. In warm-blooded animals this cannot be satisfactorily established by experiment, since the shock occasioned by so severe an operation much sooner destroys the general vitality of the system; but it may be proved in other ways to take place. After most kinds of natural death, the arterial system is found, subsequently to the lapse of a few hours, almost or completely emptied of blood; this is partly, no doubt, the effect of the tonic contraction of the tubes themselves; but the emptying is commonly more complete than could be thus accounted for, and must therefore be partly due to the continuance of the capillary circulation. Moreover, when death has taken place suddenly, from some cause (as, for instance, a violent electric shock) that destroys the vitality of the whole system at once, the arterial tubes are found to contain their due proportion of blood. Further, it has been ascertained that a real process of secretion not unfrequently continues after general or somatic death; urine has been poured out by the ureters, sweat exuded from the skin, and other peculiar secretions formed by their glands; and these changes could not have taken place unless the capillary circulation were still continuing. In the early embryonic condition of the highest animals, the movement of the blood seems to be unquestionably due to some diffused power, independent of any central impulsion; for it may be seen to commence in the vascular area, before the development of the heart; the first movement is towards, instead of from the centre, and even for some time after the circulation is fairly established, the walls of the heart consist merely of cells loosely attached together, and can hardly be supposed to have any great contractile power.

"The last of these facts may be said not to have any direct bearing on the question, whether the capillary power has any existence in the adult condition; but the phenomena occasionally presented by the fœtus at a later stage appear decisive. Cases are of no very frequent occurrence in which the heart is absent during the whole embryonic life, and yet the greater part of the organs are well developed. In most or all of these cases, however, a perfect twin fœtus exists, in which the placenta is in some degree united with that of the imperfect one; and it has been customary to attribute the circulation in the latter to the influence of the heart of the former, propagated through the placental vessels. This supposition has not been disproved (however improbable it may seem) until recently, when a case of this kind occurred, which was submitted to the most careful examination by an accomplished anatomist."

As the case alluded to, viz., that by Dr. Houston, is given in the preceding lecture, I shall not again introduce it, but pass on to the conclusions which Dr. Carpenter deduces from it. "It is evident," he says, "that a single case of this kind, if unequivocally demonstrated, furnishes all the proof that can be needed of the existence, even in the highest animals, of a capillary power, which, though usually subordinate to the heart's action, is sufficiently strong to maintain the circulation itself, when the power of the central organ is diminished. In this, as in many other cases, we may observe a remarkable power in the living system to adapt itself to exigencies. In the acardiac fœtus, the capillary power supplies the place of the heart up to the period of birth, after which, of course, the circulation ceases for want of due aëration of the blood. It has occasionally been noticed that a gradual degeneration in the structure of the heart has taken place during life, to such an extent that scarcely any muscular tissue could at last be detected in it, without any such interruption to the circulation as might have been anticipated, if it furnished the sole impelling force.

"It is equally capable of proof, on the other hand, that an influence generated in the capillaries may afford a complete check to the circulation of a part, even when the heart's action is unimpaired, and no mechanical impediment exists to the transmission of blood. Thus cases of spontaneous gangrene of the lower extremities are of no unfrequent occurrence, in which the death of the solid tissues is clearly connected with a local decline of the circulation, and in which it has been shown by examination of the limb after its removal, that both the larger tubes and the capillaries were completely pervious: so that the cessation to the flow of blood could not be attributed to any impediment, except that arising from the cessation of some power which exists in the capillaries, and is necessary for the maintenance of the current through them.

"The influence of prolonged application of cold to a part, may be quoted in support of the same general proposition; for, although the calibre of the vessels may be diminished by this agent, yet their contraction is not sufficient to account for the complete cessation of the flow of blood through them, which is well-known to terminate in the loss of their vitality. The most remarkable evidence on this point, however, is derived from the phenomena of asphyxia, which will be more fully explained in the succeeding chapter. At present, it may be stated as a fact which has now been very satisfactorily ascertained, that if admission of air into the lungs be prevented, the circulation through them will be brought to a stand, as soon as the air which they contain has been, to a great degree, deprived of its oxygen, or rather has become loaded with carbonic acid; and this stagnation will, of course, be communicated to all the rest of the system. Yet, if it have not continued sufficiently long to cause the loss of vitality in the nervous centres, the movement may be renewed by the admission of air into the lungs. Now, although it has been asserted that the stagnation is due to a mechanical impediment, resulting from a contracted state of the lungs in such cases, this has been clearly proved not to be the fact, by causing animals to breathe a gas destitute of oxygen, so as to produce asphyxia in a different manner; the same stagnation results as in the other case.

"If the phenomena which have been here brought together be considered as establishing the existence, in all classes of beings possessing a circulating apparatus, of a capillary power which affords a necessary condition for the movement of the nutritious fluid through those parts in which it comes into more immediate relation with the solids, the question still remains open as to its nature. That the capillaries possess a contractile power, far higher in degree than that of the large arteries, and more easily excited than that of the smaller, appears scarcely to admit of doubt; though to what it is due, may be reasonably questioned. It has been recently asserted by Schwann, that they possess the same kind of fibrous tissue in their walls as do the large vessels; and this cannot be regarded as improbable. It is not possible, however, that their contractility could have any influence in aiding the continuous motion of blood through them, unless it were exercised in a very different manner from that of which observation affords us evidence; for when we are microscopically examining the capillary circulation of any part, it is at once seen that the vessels present no obvious movement, and that the stream now rendered continuous by the elasticity of the arteries, passes through them as through unelastic tubes. The only method in which the contractility of the capillaries could produce a regular influence on the current of blood would be an alternative contraction and dilatation, or a peristaltic movement; and of neither of these can the least traces be discerned. Hence we should altogether dismiss from our minds the idea of any *mechanical* assistance afforded by the action of the capillaries to the movement of the blood. That the contractile coat of the capillaries has for its office to regulate the calibre of the vessels, can scarcely be doubted; but any general permanent contraction would only occasion an obstacle to the circulation,—as is shown by the effects of stimulating injections, which, if thrown into the vessels before their vitality has been lost, will not pass through the capillaries. It would appear, therefore, to be through their action on this coat that local stimuli occasion a contraction of the capillaries; their effect, however, is different from what might have been anticipated; for, instead of the capillary circulation being retarded, it is accelerated, at least until an abnormal condition results from their continued operation. Here again is another evidence that something different from mechanical power must be the agent that operates in all the foregoing cases.

"It appears from the preceding facts that the conditions under which the power in question uniformly operates, may be thus simply and definitely expressed:—Whilst the injection of blood into the capillary vessels of every part of the system is due to the action of the heart, its rate of passage through those vessels is greatly modified by the degree of activity in the processes to which it should normally be subservient in them. The current being rendered more rapid by an increase in their activity, and being stagnated by their depression or total cessation. Thus it seems that 'the capillaries possess a *distributive* power over the blood, regulating the local circulation independently of the central organ, in obedience to the necessities of each part.' If this be true, it is evident that the dilatation or contraction of the capillaries will only have a secondary influence on the movement of the blood through them. The former condition is usually an indication of diminished vital energy; and when it is observed, it is almost invariably accompanied by a retardation or partial stagnation of the current; on the other hand, the application of a moderate stimulus, which excites the contractility, accelerates for a time the motion of the blood, by rendering more energetic that reaction between the fluids and the surrounding tissues, which is the condition that really has the most influence over the current."

In the *Edinburgh Medical and Surgical Journal* for July, 1842, you will find an admirable paper by Dr. Holland, of Sheffield, on "The Forces by which the Blood is Circulated in Capillary Vessels."

The author goes through all the arguments that have been advanced to prove that the circulation through these vessels is entirely due to the force of the heart, and he shows most satisfactorily how very irreconcilable such doctrines are with facts of every-day occurrence. At the end of the paper he mentions an experiment, which I believe to be unobjectionable, and, if possible, even more conclusive than Dr. Houston's monster; it proves beyond doubt that the circulation through the capillaries is entirely owing to a vital property of these vessels, and independent of the influence derived from a *vis a tergo*. We shall allow Dr. Holland to speak for himself:—"The umbilical vein conveys arterial blood from the placenta to the fœtus, the umbilical arteries convey venous blood from the fœtus to this organ. The origin and termination of these two classes of vessels in the placenta are involved in much obscurity. No direct connexion is traced between them. Whatever opinions may be held respecting the functions of this organ, or its relation to the uterus, it will scarcely be doubted that the vein terminates in capillaries, and that the arteries originate in the same kind of vessels. It is not our intention to examine the phenomena of fœtal circulation, but to allude only to one striking peculiarity, viz., the circulation of blood in the umbilical vein. This fluid is transmitted from the placenta to the fœtus without the aid of any propulsive organ. The capillaries are, indeed, the only sources of motive power shown to exist, and hence the placenta, separated from the uterus, appeared capable of determining the influence of capillaries, and the efficiency of it in urging the blood through the long capacious vein. To institute the experiment a placenta was procured, twenty minutes after separation from the uterus, and placed, with the exception of the cord, in a bladder, which was immersed in water at the temperature of 100° Fahrenheit. The free extremity of the cord, at the same moment, was elevated to an angle of 30°, resting on the edge of a glass, and at the distance of a foot from the placenta. At the commencement of the experiment no blood escaped from the vein, but in two minutes from the immersion it began to flow, and continued for about twenty minutes, and at this time it was found that the glass had received above one ounce. Here, then," continues Dr. Holland, "is an experiment, much less exceptionable in its character than any with which we are acquainted, demonstrating the power of the capillaries to carry on the circulation, not only in their own complicated network of vessels, but in larger vessels, and which ultimately terminate in a capacious vein; and the difficulty to the motion of the blood was intentionally increased by the elevation of the whole cord above the level of the placenta. Had this organ been immersed without the bladder, the absorption or imbibition of the water would have invalidated the experiment. The water is employed as an external stimulant for the purpose of maintaining, what may be conceived to be the natural temperature of the placenta.

"The flow of blood in this experiment, in our opinion, arises entirely from the influence of the capillaries. The stimulus of the water causes the blood to excite them to contraction, and the escape of it is not opposed by any impediment. We cannot imagine that the experiment produces any important modification in the conditions of the blood. The water is not absorbed, nor is the temperature of it elevated above the heat of the body. The consideration of the circulation in this case is not complicated by circumstances acting *a tergo*, or in advance of the blood; nor by the agency of respiration, or the struggles of an animal in torture or placed in a constrained position."

In Adelon's *Physiologie de l'Homme*, vol. iii. p. 321, you will find the following remarks strongly corroborative of my view of the capillary circulation:—

"In microscopical observations on living animals," he says, "we have seen the blood in the small vessels not only circulating from arteries towards veins through the capillary systems, with such phenomena that its progress could not be ascribed to the action of the heart, but often stopping, as if hesitating on the direction which it was to follow, and even retrograding with astonishing rapidity, and for a long time. On irritating a white part, the blood is observed all at once to flow into the capillary system of this part, and this system appears to exert a sort of suction or absorbing power on this fluid."

Such, gentlemen, are some of the arguments in favour of the supposition that the capillary vessels exercise a remarkable influence over the circulation. There are other proofs which I shall not touch on at present, as the more immediate business of the hospital prevents me from deviating any further from the path of strict clinical investigation. You may ask, perhaps, why I have entered on this subject at all, or why I have dwelt so long on matters which appear to possess only a mere theoretical interest. Because I am persuaded that much error exists with respect to the nature of the forces employed in carrying on the circulation, and because I think it of the most vital importance that you should be in possession of correct principles to guide you in the numerous emergencies attendant on the treatment of disease. The human body, in its development from a lower to a higher degree of organisation, loses none of its character, it ascends, retaining in its more perfect development all that is possessed in an inferior state. In the first stage of its development it possessed a diffused nervous and vascular system. It then acquired small nervous strings and capillary vessels, and finally larger arteries, larger nerves, nervous centres, and a heart. In the same way its circulation commenced, beginning in the smaller vessels and extending to the larger, aided by the *vis a tergo*, but independent of it in a remarkable degree. From this view of the subject it follows that, in many cases of disease, we are to look to the forces which regulate the circulation of the part affected, and not to any *vis a tergo*, or propelling power of the heart. The physician and surgeon must study the life of each part in attempting to estimate its morbid conditions. It was a want of proper knowledge on this subject which led to so many errors in practice. Among these I may mention the treatment of Egyptian ophthalmia, in which it was thought necessary to drain the patient of blood for the purpose of subduing a mere local inflammation. In truth, the treatment of local inflammation, whether affecting external organs, as phlegmon, carbuncle, erysipelas, or internal parts, as pleurisy, peritonitis, &c., can never be properly understood, until the old doctrine which (by teaching that the *vis a tergo* was everything in inflammation) led to a too general use of venesection, has been laid aside and sounder opinions adopted.

FEVER.

LECTURE VII.

FEVER IN IRELAND.—EPIDEMIC OF 1847.

Before entering on the treatment of Typhus Fever, I wish to make a few preliminary observations upon its nature and peculiar characters. In the first place, typhus fever is endemic in this country; at no period, from the earliest records down to the present, has it been entirely absent—a fact of which you can easily satisfy yourselves by consulting our old authors, and by referring to the annual reports of the fever hospitals, established through different parts of Ireland. Fever, as I have said, is always endemic in Ireland, but occasionally for one year or one season, or a succession of years or seasons, it becomes much more than usually rife, and then it is said to be epidemic. In my report of the fever which devastated the west of Ireland in 1822, I advanced the opinion that such epidemics are consequent on great dearth of provisions, and their unwholesome quality. These are, no doubt, aggravating circumstances, but that they are not the sole or even the chief causes of typhus epidemics, is evident from what I have since frequently witnessed, viz., the occurrence of fever epidemics during years of plenty, of which 1826 was a remarkable example.

The epidemic fever of the last year (1847) might, to a superficial observer, appear an argument in favour of the former view, and both immediately previous to and after its commencement, this doctrine of the connexion between dearth of provisions and fever has been strongly advocated by some; but, as I shall show a little further on, this, like most epidemic visitations, may be traced to other and more immediate causes.

That fever, in Ireland at least, depends on some general atmospheric change which affects the whole island simultaneously, independent of situation, aspect, height above the level of the sea, dryness or moisture of the soil, or any other circumstance connected with mere locality, is proved by the fact, that when typhus begins to increase notably in the Dublin hospitals, we may always rest assured that a nearly simultaneous increase of fever will be observed in Cork, Galway, Limerick, and Belfast, as I have on more than one occasion ascertained by writing to the physicians of fever hospitals in these cities.

For a considerable period there was a great tendency among physicians to refer the origin of typhus, and almost every variety of fever, to malaria, or unwholesome emanations from the soil, produced by the decomposition of vegetable matter. In Ireland facts do not bear out this hypothesis; for, as already stated, when an epidemic of fever has become established, it breaks out simultaneously in situations the most different, and in some where no such emanations can be supposed to exist. Thus, I have seen a whole family affected in the telegraph, situated at the summit of Killiney, a mountain formed of bare granite, and indeed the granite and mountain districts beyond Rathfarnham, Tallaght, and Killikee, supply the Meath Hospital with its worst cases of typhus. The malarious origin of fever in general, has, I may remark, become much less probable since the publication of the official documents connected with the sickness and mortality of the British troops in the Colonies, and from which, as Major Tulloch reports, it clearly appears that fevers of the most malignant character frequently arise in places presenting, to all appearance, a combination of circumstances most favourable to the exclusion of malarious influence, while fever is never endemic in other stations, where all the reputed sources of malaria exist together.

There can be no doubt that in Ireland, as in other countries, the effects of cultivation and drainage on the health of the inhabitants are very remarkable, and I myself have witnessed several exemplifications of the improvement of the public health thus effected. Formerly ague was of rather common occurrence in some marshy districts in the immediate vicinity of Dublin, and consequently when I was a pupil, cases of intermittent fever were constantly to be met with in the hospitals; now the low grounds have been drained, and thus the production of ague has been entirely arrested. It may be cited as a proof of the former frequency of ague in Dublin, that when sulphate of quina had been discovered in France, we in Ireland were among the first British physicians who verified its *anti-aguish* powers; and Dr. Barker and I, each of us, published tables of many cases of ague cured in hospital by that remedy. If I am not mistaken, the first dose of sulphate of quina ever administered in Ireland was by myself, at the Drumcondra Fever Hospital.

It is now generally admitted that drainage greatly improves the health of the public; and this opinion has lately received additional support from the investigations of Mr. Chadwick, relative to the sanatory condition of the labouring population, from whose work the following passage is extracted :—

"In considering the circumstances external to the residence which affect the sanatory condition of the population, the importance of a general land drainage is developed, by the inquiries as to the causes of the prevalent diseases, to be of a magnitude of which no conception had been formed at the commencement of the investigation: its importance is manifested by the severe consequences of its neglect in every part of the country, as well as by its advantages in the increasing salubrity and productiveness wherever the drainage has been skilful and effectual. The following instance is presented in a report from Mr. John Marshall, jun., the clerk to the union in the Isle of Ely :—

"It has been shown that the Isle of Ely was at one period in a desolate state, being frequently inundated by the upland waters, and destitute of adequate means of drainage: the lower parts became a wilderness of stagnant pools, the exhalations from which loaded the air with pestiferous vapours and fogs. Now, by the improvements which have from time to time been made, and particularly within the last fifty years, an alteration has taken place which may appear to be the effect of magic. By the labour, industry, and spirit of the inhabitants, a forlorn waste has been converted into pleasant and fertile pastures, and they themselves have been rewarded by bounteous harvests. Drainage, embankments, engines, and enclosures have given stability to the soil (which in its nature is as rich as the Delta of Egypt), as well as salubrity to the air. These very considerable improvements, though carried on at a great expense, have at last turned to a double account, both in reclaiming much ground and improving the rest, and in contributing to the healthiness of the inhabitants. Works of modern refinement have given a totally different face and character to this once neglected spot; much has been performed—much yet remains to be accomplished by the rising generation. The demand for labour produced by drainage is incalculable; but when it is stated that where sedge and rushes existed but a few years since we now have fields of waving oats and even wheat, it must be evident that it is very great.

"On reference to a very perfect account of the baptisms, marriages, and burials, in Wisbeach, from 1558 to 1826, I find that in the decennial periods of which 1801, 1811, and 1821 were the middle years, the baptisms and burials were as under:—

	Baptisms.	Burials.	Population in 1801.
1796 to 1805	1,627	1,535	4,710
1806 to 1815	1,654	1,313	5,209
1816 to 1825	2,165	1,390	6,515

"In the first of the three periods the mortality was 1 in 31; in the second, 1 in 40; in the third, 1 in 47; the latter being less than the exact mean mortality of the kingdom for the last two years. (See Registrar-General's Second Report, p. 4, folio edition.) These figures clearly show that the mortality has wonderfully diminished in the last half century, and who can doubt but that the increased salubrity of the fens produced by drainage is a chief cause of the improvement?"

Evidence of a similar nature is given with reference to various parts of England.

In the reports given from the parish ministers in the statistical accounts of Scotland, the effects of drainage upon the general health of the population are strongly marked in almost every county, expressed in notes made from an examination of the returns. Sutherland—Parish of Rogart: "Healthy, and a good deal of draining." Far: "Subject to no particular disease; a deal of draining." Ross and Cromarty—Alness: "Dry and healthy; climate improved by drainage." It is to be understood, that drainage appears to form the essential part of agricultural improvement which is connected with the improvement of health. Thus, the notes from another parish in the same county, Kilmuir, Wester, and Suddy, state it as "healthy; great improvement; scarcely an acre in its original state." Rosmarkie: "Healthy; agriculture much improved." Elgin—New Spynie: "Healthy; much waste reclaimed, much draining." Alves: "Dry and healthy, well cultivated; wood sometimes used for drains." Banff—Deckford: "Healthy, and people long lived; much draining." Kincardine—Fordoun: "So much draining that now no swamps; formerly agues common, now quite unknown." Angus—Carmylie: "Health improved from draining." Kinross—Kinross: "Agues prevalent sixty years ago in consequence of marshes, now never met with." Oswell: "Ague prevailed formerly, but not since the land was drained." Perth—Methven: "The north much improved by draining." Redgorton: "Healthy; no prevailing disease; ague was frequent formerly, but not since the land has been drained and planted." Moneydie: "Healthy; an immense improvement by draining." Abernyte: "Since the land was drained, scrofula rare and ague unknown." Monzie: "Healthy; a good deal of land reclaimed." Auchterarder: "Much draining, and waste land reclaimed; climate good." Muckhart: "Great improvement in agriculture; ague formerly prevalent, not so now." Muthill: "Healthy; much draining, and cultivation extended." And similar statements are made from the rural districts in all parts of the country.

Ague is the most remarkable disease engendered by a marshy state of the country, and consequently the disappearance of ague forms the most easily noted and most striking change in the health of the inhabitants produced by drainage; hence ague is so often mentioned in the above extract. There is no doubt, however, that drainage not merely removes ague, but is beneficial to the public health, in removing various other maladies and derangements of the health which are observable among the inhabitants of marshy districts; and the remark made with respect to *Abernyte*, "*since the land was drained, scrofula rare,*" was, no doubt, founded on accurate observation.

Numerous other statements, corroborative of the preceding, might be easily brought forward, but though ready to allow the general improvement in the health of the public resulting from drainage, improved habits of cleanliness and increased comforts, yet I cannot admit that in Ireland we are to expect any notable diminution of continued fever from the operation of these causes. In making this statement, you are aware that I am opposing the usually prevalent opinion. The grounds for my dissent have been partly explained to you already, for, according to my observation, the increase or diminution of fever in Ireland arises from some unknown general atmospheric, or, if you will, *climatic* influences, quite independent of locality; and, consequently, the most improved and thoroughly drained towns and country districts are quite as liable to epidemics of typhus as are the most neglected and marshy parts of our island. The causes which occasion these epidemics are, on the other hand, in no way connected with the notable variations in the seasons, for with us the ravages of typhus are observed sometimes in dry, sometimes in rainy seasons; and its epidemics appear quite uninfluenced either by the cold of winter or the heat of summer. Other complaints are obviously dependent on the physical characters of the seasons, and I have made the curious observation, that whenever the weather in Dublin becomes dry and steady, the public becomes unhealthy. This singular fact admits, perhaps, of explanation; for so habituated is the Irish constitution to rapid changes of temperature, wind, and rain, that it is placed, as it were, in an unaccustomed, and therefore unnatural position, when the weather is dry and steady.

Be this as it may, the fact is undoubted, that fever is neither so prevalent nor so fatal in any of the western kingdoms of Europe as in Ireland. This opinion has been long entertained by physicians, and its truth is fully confirmed by the following extract from Surgeon Wilde's valuable report upon the table of deaths published in the Report of the Commissioners of the Irish census in 1841.

"The total deaths from fever in Ireland during the ten years included between June, 1831, and June, 1841, afforded by the census returns, amount to 112,072—in the proportion of 100 males to 86·14 females, being one death in every 10·59 of the mortality from all causes, and one in 3·4 of the deaths of the total epidemic class of diseases.

"The provincial summaries afford the following proportions of the mortality from fever, compared with the total deaths, in the different districts, and the hospitals and institutions, &c.:—

PROVINCES.	RURAL DISTRICT.		CIVIC DISTRICT.		HOSPITALS, &c.		TOTAL.	
	Epidemic.	General.	Epidemic.	General.	Epidemic.	General.	Epidemic.	General.
LEINSTER	1 in 3·25	1 in 13·02	1 in 7·29	1 in 19·53	1 in 1·09	1 in 2·32	1 in 3·21	1 in 10·85
MUNSTER	— 3·48	— 11·22	— 6·71	— 17·55	— 1·24	— 2·2	— 3·59	— 10·68
ULSTER	— 3·39	— 11·59	— 4·53	— 12·08	— 1·1	— 2·27	— 3·32	— 10·81
CONNAUGHT ..	— 3·27	— 9·34	— 6·97	— 15·64	— 1·2	— 3·13	— 3·46	— 9·79
DUBLIN CITY..	—	—	1 in 8·24	1 in 21·36	1 in 1·1	1 in 2·69	1 in 3·01	1 in 7·68
CORK CITY ..	—	—	— 6·77	— 16·75	— 2·02	— 3·77	— 4·49	— 10·51
BELFAST	—	—	— 4·01	— 10·55	— 1·06	— 1·93	— 2·5	— 6·14
GALWAY	—	—	— 7·27	— 15·98	— 1·06	— 1·38	— 4·91	— 10·45
IRELAND......	1 in 3·36	1 in 11·28	1 in 6·41	1 in 16·78	1 in 1·14	1 in 2·4	1 in 3·4	1 in 10·39

From this document it follows that the mortality from fever in Ireland amounts to a fraction less than one-tenth of the whole mortality, whereas in London the fever deaths do not amount to more than one-fiftieth of the total deaths. This difference becomes more striking from considering that deaths in Dublin from fever are actually nearly double the deaths from the same cause in London. The last census made the population of London amount to one million nine hundred thousand, whilst that of Dublin is two hundred and thirty-three thousand.

The admirable papers of Dr. Cowan have thrown much light upon the comparative frequency of fever in different parts of Britain, and his tables prove that Glasgow is more unfavourably situated, as regards fever, even than Dublin; for in 1835, 1836, 1837, the deaths from fever alone were 412, 841, 2,180, being, in the relation to the mortality from all diseases, one in 15·6, 10, and 4·7 annually; but as the year 1837 was remarkable for a fearful epidemic, this mortality is over the average, for Dr. Cowan in another place shows, that while in Glasgow, with a population of 200,000, the annual average of fever, deduced from seven years, ending with 1836, has been 1,842 cases; in Manchester, with a population of 228,000, it has been for the same period only 497; in Leeds, with a population of 123,000, only 274; and in Newcastle, with a population of 58,000, so little as 39. These numbers bring out, in striking contrast with Ireland, the immunity from fever enjoyed by large English towns, and corroborate the remark already made, that the eastern and central parts of Britain, enjoying a climate more different from that of Ireland, so likewise are much freer from fever than the western parts of Britain, whose climate approximates more to the Irish.

It is curious that in those towns in England which have greater intercourse with Ireland, as Liverpool, Manchester, Bristol, typhus predominates more than in others not similarly circumstanced. It was on this account that Dr. Lombard * concluded that maculated typhus fever was imported into England and Scotland by Irish labourers, who go over in such numbers every year to reap the harvest. But from the statistical reports of Dr. Cowan and others, it appears that, as regards Scotland, this explanation is anything but satisfactory, and it seems more probable that the west of England, Scotland, and Ireland, in which the climate is almost the same, possess the same combinations of circumstances which produce typhus. Nothing, indeed, can be more remarkable than the facility with which a simple cold (which in England would be perfectly devoid of danger) runs into maculated fever in Ireland, and that, too, under circumstances quite free from even the suspicion of contagion—in truth, except when fever is epidemic, catching cold is its most usual cause.

Much has been said and written about epidemics among cattle being simultaneous with human epidemics, and we have the testimonies of Homer and Herodotus in support of the popular belief. I am quite sure that various diseases, such as ague, remittent and bilious fever, &c., &c., may be brought on by miasmata, which, emanating from the earth, may likewise produce epidemics among cattle. Mr. Chadwick's work contains the following striking statement :—

"In the course of inquiries as to what have been the effects of land drainage upon health, one frequent piece of information received has been that the rural population had not observed the effects on their own health, but they had marked the effects of drainage on the health and improvement of the stock. Thus the less frequent losses of stock from epidemics are beginning to be perceived as accompanying the benefits of drainage in addition to those of increased vegetable production."

Dr. Edward Harrison, in a paper in which he points out the connexion between the rot in sheep and other animals, and some important disorders in the human constitution, observes :—

"The connexion between humidity and the rot is universally admitted by experienced graziers; and it is a matter of observa-

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* *Dublin Medical Journal*, vol. x.

tion, that since the brooks and rivulets in the county of Lincoln have been better managed, and the system of laying ground dry, by open ditches and under draining, has been more judiciously practised, the rot is become far less prevalent. Sir John Pringle informs us, that persons have maintained themselves in good health, during sickly seasons, by inhabiting the upper stories of their houses; and I have reason to believe that, merely by confining sheep on high grounds through the night, they have escaped the rot.

"The late Mr. Bakewell was of opinion that, after May-day, he could communicate the rot at pleasure, by flooding, and afterwards stocking his closes, while they were drenched and saturated with moisture."

The sanatory effects of road-cleansing—to which house-drainage and road-drainage are auxiliary, is, it appears, not confined to the streets in towns and the roads in villages, but extends over the roads at a distance from habitations on which there is traffic. Dr. Harrison—whose testimony has been cited on the subject of the analogy of the diseases of animals to those which affect the human constitution—in treating of the prevention of fever or the rot among sheep, warns the shepherd that, although he may provide drained pasture and avoid "rotting-places" in the fields, all his care will be frustrated if he do not avoid, with equal care, leading the sheep over wet and miry roads with stagnant ditches—which are as pernicious as the places in the fields designated as "rotting-places." He is solicitous to impress the fact, that the rot, *i.e.*, the typhus fever, has been contracted in ten minutes, that sheep can at "any time be tainted in a quarter of an hour, while the land retains its moisture and the weather is hot and sultry." He gives the following instance, amongst others, of the danger of traversing badly drained roads. "A gentleman removed ninety sheep from a considerable distance to his own residence. On coming near to a bridge which is thrown over the Barling's river, one of the drove fell into a ditch and fractured its leg. The shepherd immediately took it in his arms to a neighbouring house, and set the limb. During this time, which did not occupy more than one hour, the remainder were left to graze in the ditches and lane. The flock were then driven home, and a month afterwards the other sheep joined its companions. The shepherd soon discovered that all had contracted the rot except the lame sheep; and as they were never separated on any other occasion, it is reasonable to conclude that the disorder was acquired by feeding in the road and ditch bottoms." The precautions applicable to the sheep and cattle will be deemed equally applicable to the labouring population who traverse such roads.

With reference to this question I may remark, that although I have carefully watched the progress of fever in Ireland for more than a quarter of a century, I have not been able distinctly to connect its epidemics with any epizootic disease,—true it is, that occasionally typhus fever is prevalent at a time that some fatal epidemic affects horned cattle, pigs, and sheep, and from such an occurrence, an incautious reasoner might be led to assume a natural connexion between the two epidemics as both proceeding from the one cause. A more protracted series of observations will, however, dispel this illusion, for he will then see that the connexion is only accidental. Of this the years 1841 and 1842 afforded a remarkable example; for during both the cattle of Ireland were decimated by a most malignant epizootic, while during the same period I never recollect a greater immunity from typhus: in fact, the wards of the Meath Hospital were often destitute of a single specimen of that disease.

Before leaving this part of the subject, I will, as I promised in the commencement of this lecture, proceed now to take a short review of the fever epidemic of last year (1847), more especially of the causes by which it was produced; and conclude with a summary of my opinions.

Having made some inquiries into the prevalence of fever in Ireland in 1837—38, which I published at length in the 14th volume of the *Dublin Journal of Medical Science*, I was led to the conclusion, that the chief causes of the epidemic diffusion of fever in Ireland must be of a very general and not of a local nature, for we find the most exact agreement between results observed in cities far asunder, and widely differing in aspect and position. It must have been an influence coextensive with the island, and acting everywhere with a nearly equal degree of intensity, which brought about this coincidence, and made fever attain its maximum and minimum at the very same time in various places. It is well to keep in mind that the establishment of the existence of this epidemic influence (which in Ireland, even when at its minimum, is but too productive), does not preclude us from admitting that many other causes of minor importance may in Ireland give rise to typhus; among these we may reckon catching cold, fatigue, mental emotions, and contagion. And the result of last year's epidemic fully bears out this conclusion.

A vast amount of mischief was produced by the attempt made to connect fever epidemics with a deficiency of food; and the great diffusion, the rapid spread, and the unusual mortality which characterised the fever of 1847, must be to a great extent ascribed to the prominence which from the very first was given to famine, as an exciting cause of typhus fever in Ireland. The text put forth so authoritatively, "if there be no famine, there will be no fever," prevented proper attention from being paid to the real causes which produce and promote the spread of epidemic diseases; and the means adopted to supply a deficiency of food were, as I shall now show, singularly productive of those causes.

Want of a sufficiency, or food of an unwholesome or improper character, predisposes the human frame to disease by its debilitating effects on the system, and thus individuals become more susceptible to the contagious influence of epidemics when they exist; but I cannot admit that either cause is sufficient to generate an epidemic. Overcrowding, deficient ventilation, and filth, are causes which in themselves give rise to epidemic diseases; and when to these are added the debilitating effects of famine, we have a combination of circumstances extraordinarily calculated to promote the diffusion of a contagious disease previously in existence, and, as I have already said, typhus fever is always endemic in Ireland.

Owing to the failure of the potato-crop and other concomitant circumstances, there was great want of food in this country in 1845 and 1846. In the former year there was no remarkable increase in the number of cases of typhus fever which are at all times to be met with in Ireland, and active measures were taken to provide the people with employment and a sufficiency of food. The measures adopted had the effect of congregating together large masses in the open air in a cold wet season, and of overcrowding the poor-houses and hospitals throughout the whole country to a most frightful extent.

"Famine, also," to quote the words of Dr. Lalor in his graphic description of this epidemic as it prevailed at Kilkenny, "drove crowds of half-famished people into our large and more wealthy towns and cities, where the means of procuring food were more abundant, and the wretched and overcrowded lodging-houses, in which this class of persons found shelter, became the foci of contagion, and of the worst forms of fever. The foulness of the atmosphere was augmented by the accumulation of filth and heaps of manure and human ordure in our lanes and alleys, to an enormous and most pernicious extent; partly owing to the prevalence of bowel complaints; partly to the dearness and dearth of food, which absorbed all the time and means of our people in providing for present subsistence; and partly to the inability or unwillingness of the farmers to expend money in purchasing this manure for the cultivation of a crop so precarious as potatoes. It was in the neighbourhood of such lanes and alleys too that fever prevailed chiefly and most fatally amongst the wealthier classes."*

The congregating or crowding together of people, either in the open air or in buildings, has been at all times productive of disease. Thus in the year 1812, in the province of Gujerat, West Hindostan, the people crowded into the towns in consequence of the great destitution which prevailed, when an epidemic broke out amongst them which nearly decimated the people; but the epidemic was of small-pox, and not fever. But never, perhaps, in the history of the world was such a fearful commentary on the effects of the *entassement* of individuals witnessed as in Ireland, during the year 1847. The newspapers and periodicals of the day teemed with illustrations of the fact, *that the Irish epidemic of* 1847 *had its origin in the congregating together large masses of people at public works and at depôts for the distribution of food, and in the overcrowding the workhouses.* I shall now proceed to bring before you some of these illustrations.

Let me first refer you to the report of the Poor Law Commissioners published in May, 1847. It contains some fearfully interesting particulars respecting the progress of disease and mortality in Ireland. It appears, on reference to the report, that while the total number of deaths which occurred in the union workhouses in Ireland, for the week ending the 4th April, 1846, amounted to 159, the total number who died in the week ending the 3rd April, 1847, amounted to 2,706. A more fearful fact still is the large increase of sickness, and the large proportion of fever. The number of inmates had a little more than doubled, the numbers being on the 4th of April, 1846—50,861; and on the 3rd April, 1847—106,888; but the numbers in the hospitals increased from 8,121 to 28,239, while the numbers in the fever hospitals increased from 864 to the fearful number of 8,931. The most alarming fact disclosed by these returns is the rate of mortality which existed, and its rapid increase from the previous November. In April, 1846, the weekly rate of mortality was 3 in every 1,000 inmates. In November it showed a decided tendency to rise. During the four weeks of December it ran up from 7·4 to 8·6, then to 10·3, and then to 11. In January, 1847, it was 12·2 the first week, 13·3 the last. In February it was 17 the first week, 19·5 the last. In March it ranged from 22 to 20, and in April it rose to 25—twenty-five out of every thousand died in the last week for which there is a return.

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* *Dublin Quarterly Journal of Medical Science*, vol. v. p. 30.

In one of the local papers published about this time I find the following observations:—

"Fever has been slowly and steadily increasing in Cork for some months, and any man who calmly peruses the medical report on the state of the Cork workhouse, in February last, will feel surprised, not that fever has spread with such fearful rapidity in Cork, but that the tempestuous sweep which now appals its affrighted citizens was so long stayed. In the workhouse the inmates were put three, and four, and five in a bed, and in the convalescent ward of the hospital there were forty-five beds for one hundred and twenty persons! What result could be expected from such a state of things save that which followed?"

In other workhouses also we find the same effects to have occurred wherever they were overcrowded. It was so in Dublin, in Fermanagh, in Galway, in Limerick, in Waterford, &c. The Kilmallock workhouse, built for 800 inmates, contained on the 27th of February nearly 1,500 within its precincts. The consequence was that fever and dysentery became fearfully prevalent, and the inmates, struck with terror, began to leave the house, when the Poor Law Commissioners' sealed order against further admissions was received. That the overcrowding was the cause of the disease in this last instance there can be no doubt, for when the inmates were reduced to 1,000, in the month of April, the number of sick rapidly diminished.

The following extract which I read from a letter received in May, 1847, from Dr. Dillon, surgeon to the Co. Mayo Infirmary, and one of the poor-law guardians, is strong testimony on this subject:—"The Poor Law Commissioners have given sad proof of their ignorance of medical police, and total incompetency to direct or be connected with the sanatory state of the country —wherever their houses were in full operation, there existed disease, and *only there*. We would not open our doors and congregate poverty and filth, when we had not funds to meet its expense; we were dismissed, and held up to odium; but, thank God, we have spared human life by our decision, and have kept this locality more free from disease than any other union in the kingdom where the poor-law was in full operation; at the same time we fed our poor by private subscriptions, and lost fewer from want of food than any other place."

It is not my intention to enter into a detailed historic account of this epidemic. I am chiefly anxious to bring forward the additional proofs which it affords of the causes by which the epidemic outbreaks of typhus fever are produced in this country, and of the contagious character of the disease. No town in Ireland suffered more from this epidemic than Cork, and in no town were these causes more rife. I have now shown you to what an extent the workhouse of that city was overcrowded; and as a consequence the number of victims to fever and pestilence was frightful. From the 22nd of December, 1846, to the 24th of April, 1847—four months—2,130 persons died in the buildings comprising the union workhouse. The great increase set in about that time, and continued to the middle of March, when the curative measures of the board began to take effect, and a gradual though somewhat fluctuating decline soon took place. The following are the deaths for each week and month from December 27th:—2nd January, 59; 9th, 59; 16th, 60; 23rd, 60; 30th, 91; total for January, 329. 6th February, 128; 13th, 164; 20th, 146; 27th, 168; total for February, 606. 6th March, 143; 13th, 183; 20th, 171; 27th, 175; total for on straw in the street leading to the hospital. Another hospital capable of receiving 120 patients is about to be opened in the same neighbourhood. Whole families are now to be seen in the public thoroughfares, some stretched on straw in the sun, others lying under blankets, all disgusting-looking objects, and living on the charity of the passengers. Several batches of them were to be seen on Camden Quay during the week. Although exhibiting every appearance of outward wretchedness, many of them are impostors, as they have sums of money on their persons, and on being referred to the food depôts for relief, they indignantly refuse it. The mistaken charity of the public keeps those people within the precincts of the city, on which they have no claim whatever; and they should be sent to their own homes, as relief committees and soup depôts are now generally established throughout the country. On Wednesday a countrywoman deserted her child, which was a pitiful object, half naked, and full of small-pox, and left it in the middle of Patrick Street as a legacy to the citizens."

I cannot forbear reading for you here some judicious observations, which bear strongly on the subject I have been discussing, from the *Westminster Review* for April, 1847:—

"It is most lamentable to see that in the eagerness of impulse to apply the principle of relief, there has been, and continues to be, a total disregard of the mode. Pestilence has followed in the footsteps of benevolence, and yet death itself has awakened no suspicion of error in the aid we have given to its fearful devastations. We are told of a mortality in Irish workhouses at the rate of 70,000 per annum; but can it be pretended, with even the appearance of plausibility, that this mortality is the result of destitution? Are not the inmates of workhouses at least fed and warmed and clothed? Is there a member of the Health of Towns Association who could not tell the Government that this heavy rate of mortality can only be the consequence of overcrowding and defective ventilation? And is such overcrowding and defective ventilation to continue under a new poor-law, in the name of charity, and not to be denounced as the agency of slaughter? Let us note here a fact stated in the reports of Mr. Twisleton, that as late as the 17th of October, 29 only of the workhouses in Ireland, out of 130, were full, or nearly full; and that in the remaining 101, there was still accommodation for 34,000 inmates more than had been received. It was not till the Government expenditure upon public works had created a gigantic army of 500,000 men to swallow up all the resources of the country, that the continued rise in the price of provisions, and the desertion of families by the able-bodied, drove the feebler portion of the whole population to the workhouses as a last refuge. A last refuge indeed!—there to sicken and die. . . . And let us note again the corresponding manner in which out-of-door pestilence followed out-of-door relief, injudiciously administered. A noble lord, reading in the papers frightful tales of deaths by 'starvation,' of which he is at first incredulous, rushes from Oxford to Skibbereen, to learn the real facts by personal observation. He is taken to a cabin containing thirty inmates, all dead or dying. He sees the death-cart, and dead bodies thrown into it by callous assistants with indecent haste. He does not inquire whether plague in a hovel could, by possibility, have arisen from other causes than want. He does not see in Skibbereen a town of the better class, well situate, comparatively prosperous, but become a great centre for relief works—a focus for English charity—and therefore suddenly overwhelmed by an influx of pauperism from the surrounding districts, swarming into every kennel for nightly shelter. He heeds nothing of the evidence of sanatory reports—not even of the old and familiar history of the Black-hole of Calcutta. He reflects not that to extend the system may be to deepen the abyss of misery it has opened. He demands no modification of eleemosynary aid, but only more of that which has been afforded; and, struck with horror at that which he has witnessed, he hastens back to England—to augment the horrors."

Another mistake also made was the sudden change from a deficient and unwholesome diet to a full supply of nutritious food, which the paupers were subjected to on their admission into the workhouses. Any general change from habitual and hereditary diet, even to better, proves unwholesome, and renders the human frame more susceptible to disease. In Cork, during the epidemic, they were obliged to form an encampment for the troops, as the recruits, who joined half-famished, suffered much, and fell into bad health from the change of diet. From a somewhat similar cause, some years ago, one of the finest regiments in Sweden, consisting of Dalecarlians, lost nearly half its men. Having been ordered to the capital from their own district, the sudden change of diet from their accustomed black bread and peas to the better and more nutritious food of Stockholm so completely undermined their health, that, to save the few who escaped disease, their usual food was restored to them.

The observations I have already made are all proofs, too, of the contagious character of this fever; but its rapid spread to Liverpool and Glasgow—the two cities in Great Britain in most immediate communication with Ireland—and its subsequent progress to British America and New York, by means of the emigrant ships, can leave no doubt on this subject.

In the beginning of May, 1847, Lord Brougham presented a petition from Liverpool to the House of Lords, stating that 103,000 Irish paupers were accumulated in that town within the last six months; and soon after we find that the Irish typhus fever broke out there in all its virulence, causing very great mortality. Thus, according to the report of the Registrar-General of Mortality in England, for the quarter ending June 30th, 1847, we find that in Liverpool, in the district of St. Martin, the deaths were 661, being 200 more than in the corresponding quarter of the previous year—typhus and diarrhœa being the prevailing diseases; in Great Stewart Street district, the deaths were 1,080, a very great increase of mortality, "owing to the *Irish fever* which raged amongst the poor." In Dale Street district, "deaths 809, an increase over the previous quarter of 230, entirely owing to increase of fever amongst the lower order of Irish—280 were from fever, and 40 from small-pox." In St. Thomas district, "the deaths (598) are very considerably above the average this quarter, in consequence of the very alarming increase of fever." In Mount Pleasant district, "deaths 1,007, exceed the former quarter by 499, owing entirely to the great influx of Irish paupers into Liverpool." In Islington district the deaths were 466, an increase of 193 over the corresponding quarter of 1846; and in St. George's district "the number of deaths (188) exceeds that of any preceding quarter, and shows an increase over the corresponding quarter of 1846, of 88." And in the return for the quarter ending the 30th of September, 1847, the Registrar-General makes the following observations on the state of this great city:—"In itself one of the unhealthiest towns of the kingdom, Liverpool has for a year been the hospital and cemetery of Ireland. The deaths registered in the four quarters of 1846 were 1,934, 2,098, 2,946, and 2,735; in the three quarters of 1847, ending in September last, 3,068, 4,809, and 5,669! [to this I may add the return, since published, for the last quarter of 1847, 3,725, making the total mortality for that year 17,271]. The population of Liverpool was 223,054 at the last census. It is impossible to represent more correctly than is done by the short notes of the registrars, the piteous spectacle which this great town presented—with the floating lazarettos on the Mersey—the workhouses crowded with destitute paupers—the three large sheds, which will hold 300 persons, nearly full of patients at the present time, and the fever getting more prevalent among the upper classes."

From Liverpool the typhus fever rapidly spread throughout all the large towns in England, and it was chiefly in the *overcrowded* towns of the manufacturing districts, Manchester, Leeds, Birmingham, Sheffield, &c., and in London, that it prevailed most extensively and the mortality was greatest.

To Glasgow it was imported directly from Ireland, and there, too, the mortality was very great, the proportion of deaths far exceeding the cholera year. The mortality tables for that city for the year ending December 31st, 1847, show that the number of deaths was 18,886, an increase over 1846 of 7,250 deaths!—the great mortality arising, it is stated, from the frightful immigration of poor Irish, from whom fever spread throughout the community.

The number of emigrants who left this country, in the year 1847, for America is calculated to have been more than double that of the previous year, and, as a necessary consequence, the ships were all not only crowded but *packed* with passengers. There was scarcely a single ship in which typhus fever did not break out on the passage, and the mortality, as we might expect, was still greater than on land. From authentic documents now before me, it would appear that the number of Irish who emigrated to British North America, in 1847, was at the lowest computation 74,539; of these 5,293 are reported to have died on the passage; 8,563 were admitted into the quarantine hospital at Grosse Island, of whom 3,452 are said to have died—an average of 40 per cent.; and of those who were taken into the marine and emigrant hospital at Quebec, or who had procured lodgings in that city up to the 9th of October, there died 1,041—an aggregate of 9,786 deaths up to the period of the survivors leaving for Montreal, an average of over 12 per cent. From the account which we have had of the losses of individual ships, I am quite sure that this statement is anything but over-drawn. The "Ceylon," with 257 steerage passengers, had 30 deaths and 115 in fever on her arrival. The "Loosthank," with 349 steerage passengers, had 117 deaths, and only 20 escaped fever. Three vessels taken together lost 275 passengers. The return of the health-officers at New York shows an aggregate of 957 deaths at sea on board of vessels coming from European ports, and likewise that three-fourths of the number admitted into the quarantine hospital (most of them Irish) have been taken from British vessels.

Convincing proofs these facts of the causes of Irish typhus fever, and of its contagious character! In fine, I may state that from an attentive consideration of the last and of previous epidemics of fever in Ireland, I have arrived at the following conclusions. 1st. That epidemics of fever may occur in Ireland without any scarcity of food,—as proved by the history of many of our past epidemics. For information on this subject I would refer especially to the commentary of Mr. Wilde on the Government census of 1841, and published in the Commissioners' report. 2nd. That a scarcity may coincide with an epidemic. 3rd. As an epidemic of fever occurs at short intervals, and famine is unfortunately not less frequent, it consequently follows that an epidemic tendency to fever must frequently coincide with a visitation of famine. 4th. In 1847, as no epidemic had occurred for several years, the chances of coincidence were greater still. 5th. The contagious character of the typhus fever of Ireland was further proven by the late epidemic. Barristers and solicitors returning from circuit brought the fever to town with them. I had at one time five from Galway under my own care in Dublin; and Mr. Rynd informed me that in the convict depôt all the cases of fever came from the country. 6th. The fact of fever not often spreading in families in this city when brought from the country, proves that the causes which acted in the country were such as I have assigned—*entassement*, bad ventilation, filth, &c.

While this sheet is going through the press, we have had another practical proof of the operation of the causes I have in this lecture mentioned as productive of typhus fever in Ireland. The gaol of Galway was crowded with prisoners in the beginning of this year (1848), forced indeed to receive nearly double the number it could contain with due attention to the health of its inmates. As a consequence, fever broke out amongst those confined there, and is now spreading among the inhabitants of the town. This fact needs no comment!

LECTURE VIII.

GENERAL OBSERVATIONS ON FEVER.—CLASSIFICATION.—CONTAGION.

I HAVE already stated that when a person gets a feverish cold in Ireland, it is more apt to pass into continued fever than it is in England: this is especially the case when fever prevails as an epidemic, in which case the transition into fever takes place on account of one or other of the following causes. First—the patient had been exposed to contagion, whose effects might never have become perceptible, had not his constitution been assailed by the feverish cold. Secondly—in many cases there has been no previous exposure to contagion, and yet a feverish cold will finally determine the breaking out of fever, no doubt under the action of the prevailing epidemic influence. Thirdly—individuals who are debilitated by excesses, night-watching, and bodily fatigue are of all others the most liable to slide from feverish cold into fever: if, in addition to these causes, mental anxiety, or intellectual labour have been harassing the individual, the fever generally assumes a most dangerous form, being attended with want of sleep, raving, and often violent delirium early in the disease.

The well-known fact that individuals have sickened on the spot on smelling the effluvia from a patient's person or evacuations has led to the supposition that the contagion of fever influences the system through the nerves; and in support of this opinion many refer to prussic acid, which, *they say*, kills by its action on the nerves, and before it has been absorbed.

Another class of inquirers asserts that the blood is the seat of the first morbid change, and with equal confidence refers to the action of vegetable poisons, which they assert never produce any effect on the system until they enter the circulation.* In the present state of our knowledge it is quite impossible to determine in what manner the poison acts, and, happily, it is equally unimportant. This much is certain, that changes in the nature of the secretions, as in the sweat, sputa, mucus of the tongue, feces and urine, take place simultaneously with changes in the blood, and they are all the result of some *common unknown cause*. Of course once the blood is changed, the secretions become more rapidly altered, and when the secretions are changed, the blood is more quickly deteriorated; but the knowledge we thus obtain leads to no satisfactory explanation or practical result.

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* Blake's Experiments, *Edinburgh Med. and Surg. Journal*, vol. liii. p. 49. And Müller's *Elements of Physiology*, by Baly, 2nd edition, vol. i. p. 262.

Lately the investigations of chemists respecting the composition of the blood in fever and other diseases, have excited hopes that we are on the eve of discovering some more secure basis for our practice, founded on the analysis of that fluid. I must confess that, however I applaud these efforts of science, I entertain no hopes that they will be followed by the expected beneficial consequences: for, except the good effected in diabetes mellitus, by diminishing the quantity of starch in the bread such patients eat; and the advantage derived from medicines and articles of diet, in certain derangements of the urinary functions, such as in the phosphatic and lithic diatheses;—except in these instances, I know of no improvement in practice for which we are indebted to chemistry: and even here the result was obtained not by an examination of living, but of secreted fluids; and, in truth, it is vain to look for remedies founded on chemical principles, when these principles cannot even approximate to affording us an explanation of the mode of action of our best established medicines. When chemistry reveals why tartar emetic vomits, jalap purges, or opium causes sleep—when chemistry detects palpable changes in the blood produced by these remedies, then we may begin to hope that this science can conduct us still further, and may even, by disclosing the morbid changes which the blood undergoes in disease, become useful to us in searching for remedies capable of counteracting and even preventing these changes.

The different *theories of fever*, as they have been called, have much and often injuriously affected practice. The speculations of Brown, Cullen, Clutterbuck, Broussais, Rasori, Armstrong, and our Indian physicians, have successively introduced the stimulant, diaphoretic, general antiphlogistic, leeching, tartar emetic, mercurial plans; each of which has in its turn been pushed to a most deleterious excess. For my own part, I have long abandoned every hope of being able to frame any satisfactory theory of fever, and therefore confine myself altogether to a diligent study of its symptoms, watching how they are grouped, and in what order they follow each other, and observing closely the effects of treatment on their progress; and in my choice of remedies I am guided either by experience, or an analogy derived from the action of medicines, in other diseases which present the greatest similarity to the complications that occur in fever.

Fever in this island exhibits a great variety of character, and even during the same epidemic remarkable differences are observable, as appears from the subjoined summary, taken from Cheyne and Barker's valuable account of the epidemic fever of 1817 and 1818, vol. i. p. 425:—

"Delirium ferox was observed in Limerick, and another symptom indicating a determination of blood to the head, namely, hemorrhage from the nose, which, in some instances, took place to a very considerable extent.

"As to the organs chiefly affected in the progress of the disease, some variety seemed to exist. In most instances the brain has been reported as the organ which suffered chiefly. In some places, as at Ennis, the lungs were not at all affected during the early periods of this epidemic fever; but in other places the lungs next to the brain principally suffered; this was observed in Listowel. The same remark was made at Tralee, and Dr. Bishop observed at Kinsale that the lungs were frequently affected in children. At Ennis it was noticed as a peculiarity in the fever, that profuse perspiration occurred in its earlier stages without any relief to the patient; and it was remarked at Waterford, as stated in the report at page 251, that copious perspiration often afforded no relief. Yellowness of the skin and tunica adnata of the eyes was frequently noticed at Cork. The head and biliary system were more than usually affected.

"As the disease advanced, it was observed in most or all parts of the province, that eruptions of different kinds, either closely allied to or varieties of those termed petechial, very generally accompanied it. In some instances the eruption was papular, or a motley appearance of the skin, or a rash somewhat resembling the measles showed itself. At Cork, Dr. M. Barry remarked that, in the species of fever which he termed synochus, petechiæ seldom occurred earlier than the fourth or fifth day; but his observation, if it does not express it directly, at least implies that their occurrence was frequent. *They were generally of a bright red colour, sometimes small, at other times large.* He did not consider them dangerous, nor find it necessary to abstain from those measures of depletion which were useful when high excitement prevailed. In a communication from Clonmel, Dr. Fitzgerald states that petechiæ occurred in four cases out of five. At Fermoy, petechiæ appeared very generally among the poor. At Kinsale, a red rash, we believe of the kind above mentioned as resembling that of the measles, was common, and petechiæ were more inclined to be red than brown in that neighbourhood. At Listowel, petechiæ were so common that Dr. O'Connel did not see six cases of fever unattended by a petechial eruption, which often appeared early in the disease. The frequency of petechiæ was noticed also at Waterford, as well as of the eruption resembling measles already mentioned. The frequency of an eruption resembling measles was noticed at Bandon by Dr. Clarke and Dr. Jenkins. At Clonmel petechiæ were common even amongst children, in whose cases this eruption was not indicative of peculiar danger, but, on the contrary, often attended a mild disease. It was observed in the neighbourhood of Tramore, and we believe the same to have happened in every part of Ireland, that one member of a family had petechiæ and aggravated symptoms of typhus, whilst the relatives in the same room had fever in the mildest form. In many instances, particularly in the more advanced stages of the epidemic, the lungs were observed to suffer, as at Fermoy, Listowel, and Mallow, according to the authorities already quoted; but both at Cork and at Ennis, places very remote from each other in this province, the lungs, at least at the commencement of its epidemic progress, were but rarely affected in this fever.

"As the epidemic advanced, gastric symptoms were observed, and mention has been already made of the frequency at this time of dysentery, which, in many parts of Munster, kept pace with fever. Dr. Grogan, of Limerick, remarked that pains resembling those of rheumatism were common; and he also noticed a symptom, which there is reason to believe was not unfrequent in most parts of Ireland, namely, that the tongue, which in most febrile diseases is white or altered in colour and other appearances, in many cases exhibited no morbid change, and remained moist and clean during a great part of the disease. From the same authority we learn that increased heat of the surface, which is generally considered peculiarly characteristic of fever, was, in many instances at Limerick, altogether wanting; this absence of the usual febrile heat is observed in the worst kinds of fever."

Farther on, the report states that "Dr. Milner Barry of Cork, in his account of the fever in that city, relates that the disease presented itself under different forms, which he arranges under the following heads—1. Synochus; 2. S. Cephalica; 3. S. Pulmonica; 4. S. Hepatica; 5. S. Gastrica; 6. S. Enterica; 7. Typhus Gravior; 8. Typhus Mitior; 9. Febricula. From the arrangement which Dr. Barry here adopts, it is evident that a determination to particular organs was at Cork, as at other places, of frequent occurrence."

For more than twenty years I have in my lectures advocated the doctrine, that morbid anatomy had not served to reveal the cause of fever, which I looked upon to be an *essential* disease, or, to use the words of Fordyce, "*Fever is a disease which* " *affects the whole system; it affects the head, trunk, and extremities; it affects the circulation, absorption, and the nervous system;* " *it affects the body, and it affects the mind; it is therefore a* " *disease of the whole system, in the fullest sense of the term. It* " *does not, however, affect the various parts of the system uniformly and equally, but, on the contrary, sometimes one part is* " *more affected than another.*" "This excellent view of fever seems to be borne out completely by modern pathology, and particularly the last part, where he says that in cases of fever one part is more affected than another. We have, for instance, cerebral fevers, nervous, bilious, gastric, and catarrhal fevers, by which it is to be observed, we do not mean to imply that there is nothing more than simple disease of the brain, or nerves, or liver, or bowels, or respiratory system, but that in each of these fevers disease predominates in some particular part. So that when we speak of these fevers we speak of such a fever as Fordyce has described, in which one part of the body is affected more than the rest."*

I am happy to find that the views I have so long entertained

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* Stokes' *Practice of Physic*, American Edition, page 409.

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in opposition to the great majority of writers both in Britain and on the Continent are now generally acknowledged to be correct, as will appear by the following passage taken from the able essay on continued fever by Dr. Christison in the *Library of Medicine.*

"*Anatomical characters of continued fever.*—The pathological anatomy of continued fever remained, till lately, in a very crude and unsatisfactory condition. But no other topic has attracted so much attention during the last five and twenty years, or has been investigated with more success, so far as the accumulation of facts goes. Whether the result has been hitherto beneficial in reference either to pathological doctrine or medical practice, is a question which admits of some doubt. A very great variety of morbid appearances has been indicated as occurring in fever. Of these many are plainly incidental, because they do not by any means present themselves regularly. Others, however, have been held to be invariable; and consequently authors have sought for the nature and essence of fever, in the local morbid action which gives rise to such appearances. On taking into account the general result of the observations of all pathologists of credit, it seems impossible to avoid the conclusions, that no morbid appearance is invariable except congestion of internal organs; that every other pathological fact which has been observed is not constant, and is therefore the effect of a secondary disease; and that, in all the observations hitherto made on the pathological anatomy of fever, we must be content with discovering its *consequences, not its causes.* The information which has been amassed is important in a practical point of view, as turning the attention of practitioners to the necessity of studying and treating those secondary affections, which in various circumstances are the occasion of suffering, danger, or death. But it does not seem to throw much light on the real essence of fever; and by being rashly assumed to furnish that light, it has led to grave theoretical and practical errors." *

In fact, gentlemen, the knowledge we possess of the pathology of typhus fever is of a *negative* character. Pathology teaches us what typhus is *not*, rather than what it *is*; it shows us that it is neither cerebritis, meningitis, pneumonia, pleurisy, gastritis, or enteritis, for *it* may exist without any of these, and *they* may exist without typhus fever; but it also shows that one or other of these lesions frequently arises in the course of that fever, and these require special attention.

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* *Library of Medicine*, vol. i. p. 105.

It is difficult to classify the different varieties of fever that are observed in this city. The following are the most remarkable of the distinct varieties that have come under my notice:—

1st, Simple continued fever, without maculæ, or any notable determination to particular organs. 2nd, Continued fever, without maculæ, with determination to some organ. 3rd, Continued fever, with maculæ. 4th, Continued fever, accompanied *from the very beginning* by gastric derangement and epigastric tenderness. 5th, The last mentioned species, but in a more intense form, having black vomit and yellowness of the skin superadded. 6th, Continued fever, with petechiæ.

I have observed each of these varieties of fever constituting epidemics, which lasted for longer or shorter periods; but with us the dominant type of epidemics is the maculated form. This species, too, confers more immunity upon the sufferers than any other variety of fever, and in this respect, as well as in its well-marked eruption, it approaches in character to the exanthemata; like the exanthemata, too, this species of fever seems to be the most contagious.

Concerning contagion, the physicians of Ireland and Scotland are nearly agreed in attributing that quality to fever. The fever wards of the Meath Hospital are by no means crowded, and are both well ventilated and cleanly, while the building itself is placed in the most salubrious part of the vicinity of Dublin, being built upon the site of Dean Swift's garden; and yet it almost invariably happens that when a patient, labouring under any other acute, or any chronic disease, is admitted into a fever ward, he gets fever in the course of a fortnight, or even sooner. This happens the more surely if the patient is placed in the immediate vicinity of a maculated case. Among the pupils who attend the hospital, the greater number are sooner or later attacked by fever, and the same is true of the porters, laundry-maids, and nurses.

Moreover, in the recent epidemic with which this country was afflicted, the contagious character of the fever was, as I have already shown, peculiarly manifested; and especially by the fever hospitals, facts which strongly bear out the view that typhus fever is eminently contagious.

Another interesting point connected with the contagion of fever has lately been inquired into, viz., to determine the particular period of the disease when this character is most remarkable.

Dr. Perry, of Glasgow, was the first, I believe, who advanced the opinion, that the stage of convalescence was the most infectious in typhus fever. He considers typhus fever as a true *exanthema*. He says, "I have for some years entertained the opinion, founded upon an extensive series of observations, that contagious typhus is an *exanthematous disease*, and is subject to all the laws of the other exanthemata; that, as a general rule, it is only taken once in a lifetime, and that a second attack of typhus does not occur more frequently than a second attack of small-pox, and, judging from my own experience, less frequently than a second attack of measles or scarlet fever.

"From numerous observations and experiments, I am satisfied that it is not contagious *before the ninth day*, perhaps not till a later period of the disease. Among many circumstances which establish this opinion, I may mention one experiment which I made upon a pretty extensive scale. The fever wards of the Glasgow Royal Infirmary are each capable of containing twenty patients. The beds are arranged in two opposite rows, and are pretty near each other. While the patients are in the acute wards, they are not allowed the use of their clothes, though they may be able to sit up; they are, therefore, almost constantly confined to bed, excepting when rising to stool; and there is about one close-stool to every three patients. Into the fever-house are admitted cases of measles, scarlet fever, and small-pox; and patients are very frequently sent in labouring under bronchitis, pneumonia, erysipelas, and other local inflammatory affections. I found by experience, that when the latter class of patients were sent to the convalescent ward, where they necessarily mixed with the others, almost all those who had not a previous attack of typhus fever were either seized with it before leaving the house, or returned soon after their dismissal labouring under it; the period intervening between the time of their being sent to the convalescent ward and the attack never being less than eight days. Although means were taken to keep those recovering from small-pox, scarlatina, &c., in a separate room from those convalescent from typhus, the rooms being adjoining the non-intercourse was incomplete, and the result was, that these diseases occasionally spread among the typhus convalescents, and the convalescents from small-pox and scarlatina caught typhus. In consequence of these observations, I adopted the practice of not sending, as formerly, to the convalescent wards, those patients affected with inflammatory diseases, unless I ascertained that they were secured against the disease by having had a previous attack of typhus; but kept them in the acute fever wards till they were so far recovered as to go to their own homes, and the result was (and the practice was continued for several months), that not one of those detained in the acute wards caught the disease while there, or returned with it afterwards. From the above and other observations, I have adopted the opinion that typhus, like measles, small-pox, &c., is chiefly spread during the period of convalescence. In the paper already noticed, I have mentioned the desquamation of the cuticle which usually takes place when a patient is convalescent from typhus. Do the fine scales thrown off in this state contain the poison which, by adhering to the clothes and hair of the patient, are carried about with him, and, being rubbed off, are, while floating in the atmosphere, applied to the mucous surface, or inhaled by a susceptible recipient, in whom it produces, after a certain time, the specific disease?"*

I must here acknowledge, although frequent mention has been made in this lecture of petechial fevers, particularly in the passage cited from Cheyne and Barker's work respecting the fever of 1817 and 1818, and although, in compliance with the generally received opinions, I have set down this fever as a distinct species, that I myself have never seen petechial fever epidemic in Ireland. I was clinical clerk at Sir Patrick Dun's Hospital during the great epidemic of 1816 and 1817. The eruption consisted of maculæ, somewhat resembling measles, frequently dark and livid in bad cases; but, except in a very few instances indeed, there were no true petechiæ. In 1822 I had the charge of a large district in the town of Galway, when fever was committing great ravages; then, too, the eruption was maculated. I cannot account for so many witnesses testifying the contrary to this statement, except by supposing them to have been misled by appearances; for it must be confessed that, although *true petechiæ* are rare, *true flea-bites* are common in Ireland. Most observers, too, seem to have been very inaccurate in their phraseology, as is evident from the above quotation from Cheyne's work. Dr. Barry plainly uses the word *petechiæ* very loosely—"*They were generally of a bright red colour, sometimes small, at other times large.*" Surely this is quite descriptive of maculæ, but totally inapplicable to petechiæ; and the same may be said of the other observers, most of whom, I verily believe, overlooked the true eruption, and noted down flea-bites as petechiæ! Connected with the question first raised by Dr. Perry, whether maculated typhus should be considered as an exanthema, the fact is deserving of notice, that children exhibit the eruption much less frequently than adults, although they are quite as liable to the fever when it is epidemic. This fact is the more remarkable, because in measles, scarlatina, &c.—the true exanthemata—the eruption is more constant in children than in adults.

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* *Dublin Medical Journal*, vol. x. p. 385.

LECTURE IX.

THE GENERAL TREATMENT OF FEVER.

I SHALL to-day proceed to speak of the general treatment of fever; and in the first place I may observe that we are now at a point of time possessing no common interest for the reflection of medical observers.* It is nearly two years since my attention was first arrested by the appearance of maculated fever, of which the first examples were observed in some hospital patients from the neighbourhood of Kingstown. This form of fever has lasted ever since, prevailing universally, as if it had banished all other forms of fever, and being almost the only type noticed in our wards. Within the last four days, however, a change appears to have taken place. Scarcely any cases of maculated fever have been admitted within the last fortnight, and the majority of fever patients at present under treatment are free from cutaneous eruption so frequently observed during the last two years. The cases which we have recently admitted present no spots or maculæ, and have been termed, perhaps improperly, simple typhoid fever. And here permit me to observe, that it would be very wrong to conclude, from this circumstance, that our recent cases are of a more favourable description than those which preceded them; the disease, it is true, appears to have lost a character which is always looked upon as bad and unfavourable, but it may be just as dangerous a modification of fever as the eruptive typhus. During the predominance of the latter form, all cases without maculæ were in general simple and free from danger; but it is probable that this is not the case at present. There are two cases of this non-maculated typhus in the female ward, which are of an extremely doubtful character, and in which it would be difficult to predict the result. Indeed, were I to make any prognosis, I should say that the chances, if not against them, are at least very fairly balanced.

Now, gentlemen, as it appears we have come to a change, and that we may have to treat a new modification of fever, it behoves us to be extremely vigilant. I invite you to watch and study, with the closest attention, the cases of fever which come before you. Let us, in the first place, endeavour to ascertain whether we have seen the close of one epidemic, and are now at the commencement of another. The number of cases of simple typhoid fever has, you perceive, increased in a very remarkable manner, and the number of cases of eruptive typhus has become remarkably scarce. But there is another and a more important reason why we should study these cases with all due diligence and attention. They may be the first examples of a new epidemic, and every new epidemic, as it has its peculiar characters, so has it its peculiar treatment. We cannot follow the same track which we have pursued for the last two years—we cannot apply our remedies with the confidence of experience—we must now strike into a new path, and for some time our practice must be tentative and experimental. It was only after a good deal of experimental observation that we were able to arrive at a plan of treatment adapted to meet the exigencies of the maculated form of fever: and it is very probable that this new fever may prove at first extremely difficult to manage; and it may be some time before the diminished rate of mortality shall show that we have at length discovered its true character, and the remedies best calculated to arrest its progress.

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* The beginning of this lecture was delivered during the session 1836—7.

Let me now direct your attention to some practical points connected with the treatment of the maculated fever which has prevailed for the last two years, and which has spread to a very considerable extent in this city and its environs, attacking alike the upper, middle, and lower classes of society. It is not my intention to enter into a detailed history of the origin and progress of this fever, its varieties, symptoms, and pathological phenomena; my purpose is to furnish you with a brief but comprehensive outline of its treatment, and of the remedies which have been found most successful in its removal, as well as the most appropriate time and mode for their application.

Having made these general observations, I may observe, in addition, that in the whole range of human maladies there is no disease of such surpassing interest and importance as fever; and I cannot dwell too much on the necessity of your applying most attentively to the study of its pathology and treatment. If you compare the mortality from fever with that resulting from any other disease in this country, you will be struck with the overwhelming fatality of this affection, and will readily admit the inestimable value of a thorough knowledge of its nature and treatment. Recollect, too, that fever is a disease which numbers among its victims persons chiefly in the prime of life, and during the most active and useful stage of existence,—fathers and mothers, persons who are the ornament or the stay and support of their families, the intellectual, the industrious, the efficient, those whose lives are most valuable to their friends,—and to society. This gives an additional interest to the study of fever, and should stimulate you to endeavour to arrive at a correct knowledge of its nature and treatment.

And here let me observe, that there is nothing more untrue than the assertion, that the treatment of fever is a matter of indifference. It has been the custom to look upon every plan of treating fever as idle and absurd, and until very lately there were many persons in this country who believed that patients recovered, not from having had the advantage of treatment, but from goodness of constitution or some favourable accident; and it was usual with such persons to appeal to the experience of Dr. Rutty, who in recording the history of the epidemics of his own time (1741), observes, "the poor, abandoned to the use of whey and God's good providence, recovered, while those who had generous cordials and great plenty of sack perished." And, indeed, I must admit that the treatment of some of the cases of fever which I witnessed when a student, would seem to justify the quaint and sarcastic observation of Dr. Rutty. At that period, whether it was from bad treatment or from what has been termed the *nimia diligentia medici*, it is a fact that the maximum of mortality was among the rich, and that those who were most attended to died most speedily. In the epidemics of 1816, 1817, 1818, and 1819, it was found by accurate computation, that the rate of mortality was much higher among the rich than among the poor.* This was a startling fact, and a thousand different explanations of it were given at the time; but I am inclined to think that the true explanation was, that the poor did not get so much medicine, and that in them the *vis medicatrix* had more fairplay.* I could appeal to the practice of those times in proof of this opinion, and as we go along I shall have an opportunity of alluding to this part of the subject again, and contrasting the practice of the present day with that which was generally followed thirty years ago. If you look to Dr. Cheyne and Dr. Barker's Synopsis of the plan of treatment employed by the physicians of those days, you will be prepared, from a mere inspection of it, to admit that it was at least as hard to escape the physician as the disease. Since that period our practice has greatly improved, and things are much changed; the preponderance of fatal cases is now to be found among the poor; and the mortality among the rich, or those who have proper medical advice from the commencement, is not one-third of that which is found among the indigent, who are generally neglected at the commencement of the disease. I am therefore fully prepared to deny that, in the present state of medical knowledge, our practice is a matter of indifference; on the contrary there is no disease in which diligent attention and skilful treatment are more frequently successful than in fever, nor is there any affection of equal importance in which our therapeutic means are more efficient and valuable.

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* "The rich are less frequently affected with epidemic fevers than the poor, but more frequently die of them. Good fare keeps off diseases, but increases their mortality when they take place."—*Fletcher's Pathology*, p. 27.

Now, when called on to treat a case of fever, there are several things which require your attention. In the first place, you should examine the state of the family arrangements. This is a matter which men are apt to overlook or treat as a matter of indifference, but in my mind it is of no ordinary importance, and should be always attended to. You should never, if possible, undertake the treatment of a case of fever where the friends or relations of the patient supply the place of a regular fever nurse. The mistaken tenderness of relatives, and their want of due firmness, presence of mind, and experience, will frequently counteract your exertions and mar your best efforts. Affection and sorrow cloud the judgment, and hence it is that very few medical men ever undertake the treatment of dangerous illness in the members of their own families. The sympathy which a nurse should have for her patient should be grounded on a general anxiety to serve, and a strict sense of duty, as well as a laudable desire of increasing her own reputation ; it is, in fact, a sympathy analogous to that which should actuate a physician. Again, it will not do to have a nurse who has been usually employed in other diseases ; your assistant must be a regular fever nurse, and the man who undertakes the treatment of a long and dangerous case of fever without such an assistant will often have cause to regret it. I could mention to you many cases illustrative of the truth of this assertion. I could tell you that, where I have permitted the continuance of the services of one of the family, or of a common nurse, I have been almost invariably annoyed and disappointed. I now make it a general rule to refuse attending any dangerous and protracted case of fever without a properly qualified nurse.

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* "On the whole, the mildest and simplest treatment seems to be the most generally successful, and the result of a certain Lady Bountiful's practice forms its best commentary. She begins with an antimonial emetic; the patient is washed every morning with soap and water, gets every second day half an ounce of sulphate of magnesia, on the seventh day a blister to the neck, and if necessary some diluted wine, this seldom and sparingly; of 120 in fever treated after this mechanical plan, not one died."—*Cheyne and Barker's Report*, p. 444.

There are many nurses who are extremely attentive, but inexpert and injudicious, and their ill-judged attentions are frequently prejudicial to the patient. A fever nurse has a vast deal in her power ; if an enema is to be administered, the patient will be much less disturbed and annoyed than if it were given by an unskilful person. The mere handling of a patient—the moving of him from one bed to another—the simple act of giving him medicine or drink—the changing of his sheets and linen—the dressing of his blisters—and a thousand other offices, can be performed with advantage only by an experienced nurse. Always bear in mind that it is of the utmost importance to economize the patient's strength in fever. The very act of lifting him up, or moving him from one side to another, tends to produce exhaustion. In the advanced stages of fever, the services of a properly qualified nurse are inestimable. Then there is the moral management of the patient, and this is an office which no one can undertake unless qualified by experience, and a correct knowledge of the habits of persons labouring under such forms of disease. Every one admits the value of moral superintendence in the treatment of the insane. Now there are very few patients who are not in a state analogous to insanity, for a longer or shorter period, during a course of typhus fever. There is a necessity for moral management in fever as well as in insanity, and this is understood only by an experienced nurse. Friends or relatives are seldom found capable of discharging this office. If they chance to discover, from the physician's remarks or questions, the weak points of the patient's case, they generally contrive to let him know them in some way or other. If the patient is restless, for instance, the ill-judged anxiety of his friends will most certainly prevent him from sleeping. They steal softly to his bed, draw the curtains, move the candle so as to make the light fall on his eyes, and wake him perhaps at the moment he is settling down to rest. If he happen to take an opiate, and that they are aware of the nature of his medicine, they inform him of it, and his anxiety for sleep, conjoined with their inquiries, prevents its due operation. Hence, when you prescribe an opiate, you should not in any case say anything about it; and it should not be administered in such a way as to lead the patient or his friends to expect decided benefit from it. It is only where I have to deal with prudent persons, that I break through my rule of concealing both the nature of the medicine and the results which I expect from its operation. One of the best ways of giving an opiate is to administer it in the form of an enema. The patient's attention is then turned away from the consideration of loss of rest—he supposes that the enema is to act on his bowels, and in expecting a motion he drops asleep. You will often, too, succeed in producing sleep in this way, where you would fail in bringing it on by an opiate administered by the mouth. Another recommendation attached to this mode of exhibiting opiates is, that it can be employed in cases of delirium, where the patient obstinately refuses to swallow any kind of medicine. Let me give you here another caution. Do not let the patient know the situation or extent of his danger, however you may feel bound to act in reference to these matters towards his relatives or friends. If you apprehend mischief in the brain, do not commence by examining the head, or putting your questions in such a manner as to lead him to suspect the seat and nature of the affection. The same remark may be applied to the examination of the thorax and abdomen.

In the next place, when treating a case of bad typhus, do not think that it will be sufficient to see your patient once a day. But you will say, perhaps, that our hospital patients here do very well, and yet they are visited only once in the twenty-four hours. True—but then we have experienced nurses to look after them at all hours; we have the valuable surveillance of our apothecary, Mr. Parr; we have the attendance of the resident pupils, and of the gentlemen who take charge of the cases. You see, then, that they do not depend on a solitary visit. How often has Mr. Parr, or the resident pupil, found it necessary to change the treatment adopted at the morning visit? How often have the remedies of which we had only given a hint in the morning, been actively and energetically employed before the close of the day; and how often have lives been saved by the valuable attentions to which I have just alluded? No one should attend a case of fever without having proper medical assistants. My practice, in general, is to visit my fever patients two or three times a day; and, when I have a bad or a dangerous case to manage, I always have a competent medical assistant to stay by the patient and watch every change of his malady.

I do not know how they manage this matter elsewhere, but in this city we have so many zealous, intelligent students, so many young medical friends, and so many well-educated apothecaries, that we are never at a loss for an assistant. This fact is, I think, a sufficient answer to the objections put forward by Dr. Johnson, in the *Medico-Chirurgical Review*. He says that tartar emetic is a two-edged sword—an agent powerful alike for good or evil, and in the administration of which no ordinary circumspection is demanded. All this I am willing to admit; there is no remedy capable of producing more mischief when abused, but, when properly watched, it is, I am confident, the means of saving many valuable lives. He says, also, that Dr. Graves cannot give that share of attention to his patients which the employment of such a remedy demands. He is quite mistaken on this point. I am never at a loss for some skilful person to remain with the patient, watch the operation of each dose, and modify or change it according to circumstances. The want of proper assistants may be elsewhere an objection to the administration of tartar emetic, but this objection does not hold good with respect to Dublin.

One or two more observations of a general nature. Some persons have such a terror of foul air in cases of fever, that you will find all the windows in the house thrown open, not even excepting those of the patient's bed-chamber, and wherever you turn you are sure to meet with a current of air. Now, this is an unnecessary practice, likely to entail disease on the family, and local inflammation on the patient. The bed-room of a patient labouring under fever should be well aired, but without what is termed thorough air; and it should, if possible, be a quiet back room, away from the street. In the next place, it should be sufficiently large to hold two bedsteads conveniently; and you should order the attendants to have two well-aired beds in readiness, from one of which the patient should be changed to the other every twelve or twenty-four hours. You can scarcely have an idea of the comfort this affords to a person in fever. The room can be kept properly ventilated by a fire, and the temperature can be regulated by a thermometer. Some persons are in the habit of constantly sprinkling the room with vinegar—others with the chlorides. I do not know that it is necessary, and I think that the use of chlorine is doubtful, if not improper, and may prove injurious to the patient.

Having made these few general observations on the steps to be taken by those who enter on the treatment of typhus, I shall now proceed to speak of diet and medicines. In a disease like fever, which lasts frequently for fourteen, twenty-one, or more days, the consideration of diet and nutriment is a matter of importance, and I am persuaded that this is a point on which much error has prevailed. I am convinced that the starving system has, in many instances, been carried to a dangerous excess, and that many persons have fallen victims to prolonged abstinence in fever. This was one of the errors which sprung from the doctrines of those who maintained that fever depended on general or topical inflammation. They supposed that fever arose from inflammation, and immediately concluded that, to treat it successfully, it was necessary to reduce the system by depletion and low diet, and to keep it at this point during the whole course of the disease. Hence the strict regimen—the *diète absolue*—of the disciples of the physiological school, and of those who looked on inflammation as the essence of fever. The more the symptoms appeared indicative of inflammatory action, the more rigorous was the abstinence enforced. If a patient's face was flushed, or his eyes diffused, no matter what the stage of the fever was, they said, "Here is inflammation of the brain, and nourishment will exasperate it." If he had red or dry tongue, and abdominal tenderness, they immediately inferred the existence of gastro-enteritis, and all kinds of food, even the lightest, were strictly forbidden. That this proceeds from false notions on the nature of fever is beyond doubt, and I pointed out this fact many years ago, long before the appearance of Piorry's work. Let us, in the first place, examine the results of protracted abstinence in the healthy state of the system. Take a healthy person and deprive him of food, and what is the consequence? First, hunger, which after some time goes away, and then returns again. After two or three days the sensation assumes a morbid character, and instead of being a simple feeling of want and a desire for food, it becomes a disordered craving, attended with dragging pain in the stomach, burning thirst, and, some time afterwards, epigastric tenderness, fever, and delirium. Here we have the supervention of gastric disease, and inflammation of the brain as the results of protracted starvation.

Now, these are in themselves very singular facts, and well deserving of being held in memory. Read the accounts of those who perished from starvation after the wreck of the "Medusa" and the "Alceste," and you will be struck with the horrible consequences of protracted hunger. You will find that most of the unhappy sufferers were raging maniacs, and exhibited symptoms of violent cerebral irritation. Now, in a patient labouring under the effects of fever and protracted abstinence—whose sensibilities are blunted, and whose functions are deranged—it is not at all improbable that such a person, perhaps also suffering from delirium or stupor, will not call for food, though requiring it; and that if you do not press it on him, and give it as medicine, symptoms like those which arise from starvation in the healthy subject may supervene, and you may have gastro-enteric inflammation, or cerebral disease, as the consequence of protracted abstinence. You may, perhaps, think that it is unnecessary to give food, as the patient appears to have no appetite and does not care for it. You might as well think of allowing the urine to accumulate in the bladder, because the patient feels no desire to pass it. You are called on to interfere, where the sensibility is impaired, and the natural appetite is dormant; and you are not to permit your patient to encounter the horrible consequences of inanition, because he does not ask for nutriment. I never do so. After the third or fourth day of fever, I always prescribe mild nourishment, and this is steadily and perseveringly continued through the whole course of the disease.

Again, let us see how close a resemblance the symptoms generated by long-continued denial or want of food bear to those which are observed in the worst forms of typhus. Pain of the stomach, epigastric tenderness, thirst, vomiting, determination of blood to the brain, suffusion to the eyes, headache, sleeplessness, and, finally, furious delirium, are the symptoms of protracted abstinence; and to these we may add, tendency to putrefaction of the animal tissues, chiefly shown by the spontaneous occurrence of gangrene of the lungs. It has been shown by M. Guislain, physician to the hospital for the insane at Gand, that in many instances gangrene of the lungs has occurred in insane patients who have obstinately refused to take food. Out of thirteen patients who died of inanition, nine had gangrene of the lungs. You perceive, then, that starvation may give rise to symptoms of gastric disease, to symptoms of cerebral derangement, and to mortification of the pulmonary tissue. It is not, therefore, wrong to suppose that when a system of rigorous abstinence has been observed in fever, and when food has been too long withheld, because, forsooth, the patient does not call for it, and because his natural sensibilities are blunted and impaired—it is not, I say, unreasonable to infer that gastric, cerebral, and even pulmonary symptoms may supervene, analogous to those which result from actual starvation.*

An attentive consideration of the foregoing arguments has led me, in the treatment of long fevers, to adopt the advice of a country physician of great shrewdness, who advised me never to let my patients die of starvation. If I have more success than others in the treatment of fevers, I think it is owing in a great degree to the adoption of this advice. I must, however, observe that great discrimination is required in the choice of food. Although you will not let your patient starve, do not fall into the opposite extreme: you must take care not to overload the stomach. When this is done, gastro-enteric irritation, tympanitis, inflammation, and exasperated febrile action are the consequences. I have witnessed many instances of the danger of repletion in febrile diseases. A case of this kind occurred some time ago in this hospital, in a boy who was recovering from peritonitis. In another case, in private practice, an incautious indulgence in the use of animal food was followed by a fatal result. A young lady ate some beefsteak, contrary to my orders, at an early period of convalescence from fever, relapsed almost immediately, and died of enteritis in thirty-six hours.

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* Huxham gives the history of a gentleman who obstinately starved himself to death, and would not for many days, either by force or persuasion, swallow any kind of food, or a drop of liquor. He soon grew feverish, flushed in his face, and very hot in his head; his pulse was small but very quick, in four or five days his breath became exceedingly offensive, his lips dry, black, and parched, his teeth and mouth foul, black, and bloody, his urine vastly highly-coloured, and stinking as much as if it had been kept a month; at length he trembled continually, could not stand, much less walk, raved and dozed alternately, fell into convulsive agonies frequently, in which he sometimes sweated pretty much about the head and breast, though his extremities were quite cold, pale and shrivelled; the sweat was of a very dark yellow colour, and of a most nauseous stench.

Food must be given with great care and judgment, particularly in the beginning of fever. For the first three or four days, particularly if the patient be young and robust, water, weak barley-water, and whey will be sufficient. After this it may be well to begin with some mild nutriment. What I generally give is some well-boiled gruel, made of groats and flavoured with sugar, and if there be no tendency to diarrhœa, a small quantity of lemon juice. The ordinary oatmeal gruel does not answer sufficiently well for this purpose, for it is apt to produce griping and diarrhœa—symptoms which are extremely disagreeable in the commencement of fever, and which often lead to others of a more troublesome and formidable character. I am also much in the habit of ordering a little thin panado, morning and evening, during the latter part of the first, and the beginning of the middle stage of fever. A small slice of bread is slightly toasted, and boiling water poured on a tablespoonful of the crumbs, in sufficient quantity to make a thin panado, of which the patient takes a tablespoonful two or three times a day. It may be flavoured with a very small quantity of lemon juice and sugar, if there be no tendency to diarrhœa; but where this exists, or where you are administering mercurials, I think you should be cautious in the use of acids. Although medical men of the present day do not object to giving acids during the use of mercurials, I think the practice is not entirely devoid of danger, and I think our predecessors were right in withholding them under such circumstances.

bitter, slightly acidulated, will appease the morbid thirst of fever more effectually, and for a much longer period, than large draughts of water, or any of the fluids usually employed for the same purpose. You should always bear in mind, that thirst in fever does not exclusively depend on a dry or parched state of the mouth or fauces, but lies much deeper in the system, and has its origin in some peculiar derangements of the nerves, most probably of those belonging to the ganglionic system. In going through a fever ward, you meet with numerous illustrations of the truth of this position; one man, with a moist tongue and fauces, labours under insatiable thirst, while you will observe another with parched tongue and throat, and yet without any desire whatever for fluids, or any choice as to their temperature. We had two examples of this in the fever ward during the past week. One patient with a moist tongue was incessantly calling for drink, while another man, who had his tongue almost perfectly dry, exhibited a very remarkable indifference to fluids.

One general observation as to the administration of food and nutriment in fever. All kinds of food and nutriment should be given by day, and the patient should, if possible, be restricted to the use of fluids by night. The natural habit is to take food by day and not by night, and in sickness as well as in health we should observe the diurnal revolution of the economy.

When you give nutriment, then, be careful in observing the usual periods of meals. The space of time to which I limit the giving of chicken broth, jelly, arrowroot, and other mild articles of diet, is from eight o'clock in the morning to eight in the evening. Always make it a rule that your patient shall take nutriment within the space of those twelve hours during which he is accustomed to take his meals when in health, and allow him nothing but mild diluent fluids during the night. I am persuaded that I have seen much benefit derived from following this simple plan.

With respect to drinks, the mildest, of course, should be preferred: on this point most persons are generally agreed, and it will be unnecessary for me to detain you with any particular observations. There is one error, however, which is very frequently committed in the use of drinks in fever; patients are generally allowed to drink too much. It may be urged that they have a strong desire for fluids; but they should not be gratified in everything they wish for. They labour under a constant state of nervous irritation and restlessness, and will beg of you to do twenty different things to relieve their immediate feelings; but it would be just as improper to give them large quantities of drink every time they desire to call for it, as to indulge them in any momentary whim which may be the offspring of their disordered and changeable fancy. The continued swilling of even the most innocent fluids will bring on heaviness of stomach, nausea, pain, and flatulence, and predisposes to congestion and intestinal irritation. From the mere ingestion of a large quantity of the simplest fluid, you will frequently see well-marked symptoms of gastric irritation arise during the course of fever. This is not a picture drawn from imagination; I have witnessed it on many occasions during the course of my practice. It is extremely painful, indeed, to be obliged to refuse drink to a patient labouring under intense thirst; but you should never allow them to take a large quantity of fluid at a time; you should impress upon them the danger attendant on such a practice, and tell them that a spoonful or two, swallowed slowly, allays thirst more effectually than drinking a pint at a time. The sensation of thirst, as you all know, is almost entirely confined to the fauces and upper part of the pharynx, and it is as much relieved by a small quantity, swallowed slowly and gradually, as it is by a large quantity gulped down at once.

Besides the simple fluids, there are other drinks required in fever. Beer, ale, porter, wine, tea, and coffee are also frequently used in the treatment of fever, and are of the utmost value when employed on appropriate occasions; they are adjuvants of the highest importance in the dietetic management of fever, and it will require some time to explain the rules by which you should be guided in their administration. I shall therefore speak of them according to the indications with which they are given; and first of tea and coffee.

You are aware that we give sedatives and narcotics to tranquillize, to produce a species of exhaustion of the mental faculties, and to bring on sleep; and I do not see any reason why we should not also administer expergefacients, or remedies calculated to maintain intellectual activity, and keep the patient awake. Among the remedies most frequently employed for the latter purpose are tea and coffee. You have lately seen an infusion of green tea useful in a case of narcotism which occurred in the fever ward. A man in the latter stage of fever, and labouring under great nervous excitement and total loss of sleep, was ordered an opiate enema, after we had tried various other means without success. During the course of the evening he got twelve drops of black drop, with two ounces of mucilage of starch, in the form of enema, and soon after fell into a sound sleep. When we came next morning and inquired after him, everything was reported to have gone on well; the opiate enema had answered the purpose completely, and the man was still sleeping deeply. We found, however, on a more accurate examination, that he was in a kind of lethargic state and could scarcely be roused. When addressed in a loud tone of voice, he raised himself heavily and slowly, half opened his eyes, gave a brief answer to our questions, and then, leaning back on his pillow, dropped asleep. Observe here the danger connected with this state. He was in an advanced stage of fever, had been restless and sleepless, and had suddenly passed to an opposite state. The rapidity with which coma had supervened on sleeplessness, and the danger of fatal congestion of the brain coming on, gave me considerable alarm. There was no use, however, in thinking of what had been done; the man's state called for prompt and decided measures, and we proceeded at once to attack the symptoms of our own creation. One of the gentlemen went down and got some green tea, of which he made a strong infusion, and administered a strong dose of it to the patient. This had the desired effect; the symptoms of coma gradually disappeared, and when I came to see him in the afternoon, he was quite out of danger. Green tea was first introduced here as an expergefacient in the treatment of coma by Dr. Edward Percival, son of Dr. Percival of Manchester; and some years ago he read a paper at a meeting of the College of Physicians, in which he brought forward several cases of coma and stupor, in which green tea had produced the most favourable effects. On the Continent they generally use strong coffee for the same purpose. Whether these beverages produce this effect by their influence on the circulation, or on the nervous system, I am not prepared to say; but there cannot be a doubt of their efficacy and value in many cases of this description; and I am frequently in the habit of using both with this intention.

While on the subject of expergefacients, I shall beg leave to read for you a very curious case from the 13th number of the *Boston Medical and Surgical Journal*, in which an expergefacient of a less agreeable character was employed to rouse a patient from the lethargic stupor brought on by a large dose of laudanum. There are some transatlantic peculiarities of expression in the details of this case, but I have no doubt of its being correct. It is entitled "a case of successful treatment by flagellation, where a large dose of laudanum had been taken." And the author, Dr. Joseph Barrett, of Middleton, Connecticut, proceeds as follows:—

"Tincture of opium is not unfrequently resorted to for destructive purposes. It is also, unfortunately, and too frequently, taken by mistake, and proves fatal before efficient means can be adopted to counteract its deleterious effects on the system. I am induced, therefore, to offer a short statement of a case of poisoning with laudanum that fell under my care several years since, for the following reasons: first, the success that attended the mode pursued; and, secondly, not having met with any such means recorded, to my knowledge,* either in works on medicine, or in treatises on poisons."

Observe, it is not I that am speaking here, but Dr. Barrett, of Middleton, Connecticut.

"In the year 1822, February 23rd, I was called on to see Mr. Wright Harris (this was in the State of New York), who had intentionally taken a large dose of laudanum for the purpose of destroying himself. He had committed this act during his absence from home, under circumstances which it is not important to relate. Much time (about three hours) was therefore lost before any effectual measures could be adopted for his relief. His case, as I found him, appeared to be altogether hopeless. Before my arrival, emetics and various drinks had been tried, besides frictions, and constant though ineffectual attempts had been made to irritate the œsophagus by feathers. All these means had failed, and the patient was in such a profound sopor, that apparently nothing but warmth remained to indicate that life had not already become extinct. The quantity of laudanum taken was ascertained to be one ounce and a half. The case appearing so desperate, justified me in the course of treatment which I was, under existing circumstances, then obliged to adopt.

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* This practice, though not generally adopted, has been recommended by several authors in Europe.

"Internal remedies having entirely failed, there was no chance left but for high external excitements. I therefore determined to use vigorous measures. I commenced with flagellations, using long, pliant, fresh twigs to the palms of the hands and soles of the feet. These were briskly applied, and in a short time gave indications of uneasiness and pain. This treatment was unremittingly pursued till the man spoke, and complained of being pained by the whipping, when this severe appliance was relaxed; but on so doing, he instantly sunk into a profound stupor, from which he was again only roused by the severity of the whipping. It required the aid of a number of men to take turns in the flagellation, as well as to support and walk him about, for a cessation of the use of the rods was followed by instantaneous stupor. After about six or eight hours under this course, the stupor was lessened, and the severity of the flagellation mitigated; but, as the case required constant high excitement it was still repeated at intervals, till eventually the exercise of walking was sufficient to keep him awake. This was in about twelve hours from the commencing with the flagellation. He afterwards experienced but little inconvenience from his hands and feet, and was perfectly restored in a few days to his usual health. I would here state that the first proposal made by me to adopt flagellation, as the only hope, was objected to by persons present, from its carrying with it the semblance of unkindness towards what was regarded by them as a corpse; and it was not till the application of the rods by myself in the first instance, that I obtained the aid of those present; but as soon as the patient began to move, and at last spoke, they took hold with alacrity, and, by dividing themselves into relief parties, they very cheerfully, and rather amusingly, kept up the castigation so long as the state of the patient required it at their hands. He by no means seemed to relish this harsh proceeding, and in return gave his attendants several severe blows. If, while lifting his arm to give a blow, the flagellation was then entirely suspended, the arm would instantly sink powerless; to such a degree had the effects of the narcotic drug prevailed over the nervous system, that nothing but the torture of the rods could

LECTURE X.

THE GENERAL TREATMENT OF FEVER.—TYMPANITIS.—HICCUP.—HEMORRHAGE FROM THE BOWELS.

BEFORE I proceed to speak further of the diet and remedies to be employed in the treatment of typhus fever, allow me to make a few observations. There is a patient at present in the fever ward, whose case shows the necessity of strict attention and incessant watchfulness on the part of those who have the management of bad cases of fever. A man who has been labouring under delirium, with symptoms of cerebral excitement and congestion, was ordered the tartar emetic solution, with the view of reducing the increased vascular action; but on inquiry this morning, we find that he has taken no medicine, and that his symptoms have been allowed to go on unchecked for twenty-four hours. He refused to take his medicine, and the nurse very improperly neglected to report the circumstances of the case, in order that proper steps might be taken to remedy so dangerous an omission. Thus a whole day has been lost at a most critical and important period of fever. There can be no excuse for such negligence as this, for it could be easily remedied. Patients in this state have always more or less thirst, and a spoonful of the tartar emetic solution could be mixed with whey or cold water, and administered in this way without his knowledge, or, if he refused to drink any fluid, it might be given in the form of enema. There is no excuse, therefore, for such negligence; and when you recollect the state that such patients are in—their nervous excitement, incessant raving, agitation, struggling, and sleeplessness—you will be able to appreciate the dangerous and even fatal consequences that may arise from culpable neglect of this kind.

At our last meeting I spoke of the use of food and drink, and laid before you my views of the most appropriate articles of diet in the various stages of fever. I told you that I attributed much importance to the use of a proper regimen, and that I looked upon the observance of this principle as a main cause of success in the treatment of typhus. I think it is chiefly owing to our care in this respect that so few of our patients have tympanitis. Now and then we have cases of fever with tympanitis and diarrhœa, but in the majority of instances, these are persons who have been under treatment before admission, and who have been too much purged. The use of drastic purgatives in the early and middle stages of typhus is one of the most fertile sources of subsequent evil, and there are few evils of greater magnitude than tympanitis with diarrhœa, and gastro-enteric inflammation, particularly in the latter stage of fever. Now, if you inquire into the history of the cases in which these symptoms are most distinctly marked, you will find that in at least two-thirds, powerful cathartics have been employed not once, but repeatedly, in the commencement of the disease. Almost all cases in which calomel and colocynth, or aloes, followed by black draught, have been liberally used in the commencement, become tympanitic, and frequently at a very early period.* The same mischief, but

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* The views of Dr. Stokes quite agree with mine:—

"A common practice has prevailed in these countries, and, indeed, still exists to a very great extent, of making the patient take a purgative medicine every day; and this, I regret to say, is too often done, even in cases where the surface of the small intestine presents extensive patches of ulceration. Now, I will ask you, can anything be so barbarous as this, or can it be exceeded in folly or mischief by the grossest acts of quackery? Here we have an organ in a state of high irritation, and exhibiting a remarkable excitement of its circulation; and yet we proceed to apply stimulants to that organ, and to increase the existing irritation. Would it not be absurd in a case of inflammation of the knee or elbow-joint to direct a patient to use constant exercise and motion? Would it not be a very strange practice to apply irritants to a raw and excoriated surface? Yet something equally absurd, and equally mischievous, is done by those who employ violent purgatives in a case of inflammation of the digestive tube in fever. This has been the great blot in the history of British practice. Calomel, and black bottle, and even jalap, and aloes, and scammony, have been prescribed for patients labouring under severe and extensive dothinenteritis. Morbid stools are discharged, and the more morbid they are, the more calomel and purgatives does the physician give to change their character, and bring them back to the standard of health. I want words to express the horrible consequences. Too often have I seen fever patients brought into the hospital with diarrhœa, hypercatharsis, and inflammation of the mucous membrane, from the use of purgatives administered before their admission. Practitioners will not open their eyes. They give purgatives day after day, a very easy practice, and one for which there are plenty of precedents; but it is fraught with the most violent consequences. I will freely admit that the disciples of the school of Broussais have gone too far in decrying the use of laxatives altogether; but if they have lost hundreds by this error, British practitioners have *killed thousands* by an opposite plan of treatment. In cases of fever, where there is in a less degree, is apt to occur where a system of strict abstinence has been enforced and continued undeviatingly for a considerable length of time. Want of food, even in the healthy state of the system, is apt to produce flatulence, weakness, and distention of the stomach; and in many instances gives rise to very serious forms of gastro-intestinal irritation. The *diète absolue* is very apt to produce the same effect in fever. Even the abuse of drinks of the simplest and most innocent description is apt to produce flatulence, distention, and a tendency to tympanitis. Hence the value of the rule which I laid down in my last lecture, viz., to allow the patient only small portions at a time, and to order him to swallow them slowly. The abuse of the ordinary drinks, as common water, whey, barley-water, soda and seltzer waters, and effervescing draughts, is a frequent source of tympanitic swelling in fever.

Having commenced the subject of tympanitis in fever, I cannot do better than proceed now to describe its causes and the mode of treating it which I have found most effective.

The mucous membrane of the alimentary canal secretes air in great abundance during health. The immediate uses of the secretion have not been enough studied, nor have I now sufficient time to dwell on this subject; it may be remarked, however, that the presence of air in the bowels must be of great importance, both physically and chemically assisting digestion, which essentially consists in the gradual softening and final solution of the solid food, and the absorption of the dissolved portions. Physically the air must facilitate the motions of the alimentary bolus, keeping the bowel in a suitable state of distention, and being ready immediately to occupy the place of the solid or fluid contents as they are moved about or absorbed; chemically, it is well known that certain gases, such as carbonic acid—a gas

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no decided symptom of gastro-enteric disease, there can be no objection to the use of laxatives, *if required*, but they should always be of the mildest description. You will gain nothing by violent purging in fever, mild laxatives alone can be employed; and where there is any sign of intestinal irritation present, even these should be used with caution. There is one way of opening the bowels, which you may always have recourse to with advantage in fever, viz., the use of enemata. There is not the slightest doubt that occasionally accumulations of fecal matter will take place, and tend to keep up irritation; but they should always be removed with the least risk of producing bad consequences. To purge in fever when intestinal irritation is present is a practice opposed alike to theory and experience, and I have already stated that its results are most horrible."—*Dr. Stokes' Lectures*, American edition, p. 500.

where else. If, in such a case, the stomach is weak and unable to perform its functions, the remaining part of the digestive tube is strong, and pours out the fluids necessary for completing the process with great energy.

Again, we meet with many persons who never complain of acidity, pain, flatulence, or sense of distention and weight in the stomach, and yet they are frequently annoyed with unpleasant abdominal sensations; they have costive or irregular bowels, diarrhœa, tormina, tympanitis, fetid, unhealthy evacuations, and scanty, high coloured urine. They feel uncomfortable, not immediately after a meal, but in three or four hours; they lose flesh and strength, and have a pale, sallow, unhealthy look. Here the dyspepsia is intestinal; the stomach works well, and performs its functions with vigour, but when the alimentary mass enters the small intestines, it produces a great deal of discomfort, because the supplementary digestion is deranged, and its performance attended with much labour and difficulty.

In some cases both these forms of dyspepsia are combined, and these are, of course, the worst; but they exist quite distinct from each other, and a patient, with his stomach in a perfectly normal and healthy state, may labour under dyspepsia from derangement of the digestive functions of the small intestines; or, with the latter in a healthy state, he may have indigestion from simple gastric derangement. We have, indeed, reason to conclude, that when organic or functional disease so impairs the energies of the stomach that it assists but little in the performance of digestion, the intestinal digestion becomes more intense; it is only thus that we can account for the absence of emaciation in certain cases, such as that of Napoleon Buonaparte, where, nevertheless, the stomach was so extensively disorganized as totally to prevent its taking any part in the process of digestion.

The preceding remarks, though not directly connected with, are nevertheless illustrative of the subject under consideration—it being evident that the secretion of air natural to the mucous membrane of the intestines during health, may readily be augmented in disease, so as to give rise to intestinal tympanitis. This happens in all cases where inflammation or congestion attacks this tissue—an occurrence particularly frequent in fever. When tympanitis takes place in the commencement of fever, it invariably proceeds from inflammation, and is usually preceded by tenderness and other unequivocal symptoms of inflammatory action within the abdominal cavity. The remedy for this complication consists in local blood-letting freely applied together with small doses of Dover's powder, and considerable doses of hydrargyrum cum cretâ: all active aperients should be avoided, but emollient lavements are often useful.

When tympanitis occurs during the middle or latter stages of protracted fever, it is sometimes inflammatory, but more frequently depends on a state of venous congestion; occupying a considerable extent of the mucous membrane of the small intestines, which subsequently becomes gorged with blood, and livid, and secretes, among other morbid matters, a large quantity of gases. This tympanitis is often preceded by bowel complaint, unaccompanied by abdominal tenderness or pain, in the first instance—a state of things which may last for one or several days before inflation of the intestines commences. When this occurs, then, if it proceeds rapidly, the belly becomes painful and somewhat tender on account of the sudden distention; and a superficial observer is thus apt to attribute the tympanitis to active inflammation.

Now, as this state of things takes place at a period of great debility, when the powers of life are already much exhausted, and when even the application of a few leeches may be followed by alarming weakness, it is evident that this tympanitis must be treated in a manner different from that above spoken of. In general, it will be right to commence with the exhibition of ten or fifteen grains of magnesia, with the same quantity of rhubarb, given in some carminative vehicle, such as spearmint or fennel water; after this has operated, the belly should be well stuped, and rubbed with a stimulating terebinthinate liniment. It often happens that, after the operation of the rhubarb, the diarrhœa, and with it the tympanitis, begins sensibly to diminish, and then a little care soon removes these symptoms altogether. Sometimes, however, no such improvement follows; and the belly continues to swell, while the bowel complaint is unchecked. This is a dangerous crisis, and requires the utmost judgment in its treatment.

It is of great consequence to remark, that when the bowel complaint, has preceded intestinal tympanitis in fever, and when, notwithstanding the continuance of the bowel complaint, the tympanitis has gone on increasing, oil of turpentine will seldom be of the least use, whether exhibited by the mouth or in an enema. We must, therefore, under these circumstances, look for some remedy different from those usually recommended, and such remedy we possess in the acetate of lead.

Pathologists are agreed that venous congestion and active inflammation of the mucous membrane of the intestinal canal may often be associated together; and, in fact, although these two states are different, and require different remedies, yet they so nearly approach each other as to require medicines taken from the class of antiphlogistics; the one requires, however, a very different antiphlogistic from the other, just as chronic dysentery must be combated by remedies different from those suited to acute bowel complaints. Oil of turpentine is admirably suited to the cure of congestive tympanitis in fever, where no bowel complaint, or a very slight one, has preceded or accompanied it. But is oil of turpentine an antiphlogistic remedy? I answer, does it not cure certain cases of iritis, of sciatica, and of epilepsy? When, however, a bowel complaint forms the chief feature in a patient's state, and is associated with tympanitis, then the acetate of lead must be our sheet anchor.

I was first led to use this medicine in considerable doses, in the latter stages of protracted fever, on the recommendation of Dr. Bardsley, for the purpose of preventing that state of the bowels which so insidiously leads to ulceration of Peyer's glands. Dr. Bardsley certainly deserves much credit for the introduction of this remedy, with which I became familiar in consequence of using it largely in Asiatic cholera—a disease in which the serous discharges are almost invariably preceded, and, when the patient recovers, invariably followed, *by a copious secretion of air into the bowels*. This it was that led me to observe the anti-tympanitic properties of the sugar of lead; for I have found it to be *a remedy, not merely for the secretion of serous fluid into the intestines, but for the secretion of air in that disease*. Afterwards, analogy led me to apply it to the cure of tympanitis combined with diarrhœa, in the middle or latter stages of fever; and I have had much reason to congratulate myself upon this new application of the remedy, for it has been very successful in my hands. It may be well to observe that sugar of lead, besides its intestinal hemorrhage occurring in fever. A person in fever gets increased frequency of pulse, heat of skin, dry tongue, and about the twelfth day his head becomes engaged, his countenance flushed, eyes suffused, and a tendency to sensorial derangement. His bowels at the same time are affected, and tympanitis appears. Matters then grow worse, he begins to pass blood, and, on visiting him, his alarmed relatives show you quantities of thin grumous blood which he has discharged from his bowels. Now, what course are you to pursue in this case? Stop all medicines whatsoever, and let your patient alone. Watch the progress of this discharge, and you will find that it disappears gradually, and, when this occurrence takes place, never do anything. As in fever a patient may get epistaxis, and it may usher in a favourable crisis, so, in like manner, he may have a critical discharge of blood from the bowels. In either case, you are not to interfere with the wise provisions of nature, or to give anything which may produce irritation, or cause a cessation of this salutary process. You recollect a case of this kind in the hospital, which the students requested me to stop, and that I refused to do so, because I thought the hemorrhage critical. But it may happen that this sanguineous flux may go on so far as to threaten great danger. This is certainly an occasional result, for I have seen epistaxis terminate fatally. Here you must interfere to avoid a greater evil; and it is at this critical period that the internal exhibition of oil of turpentine combined with opium may be ventured on; but while the bleeding continues moderate, and exhibits no threatening indications, and is accompanied by a corresponding diminution of fever, you should leave the matter entirely to nature. You perhaps have seen a patient here, who on the fourteenth day of fever got this discharge of grumous blood, and may remember that we gave nothing but a little of the saturated solution of carbonate of ammonia. Now, if we had given this patient an opiate, we should have repressed a sanatory effusion, or, if we had given him a purgative, we might have precipitated it into a fatal hemorrhage.

I shall next proceed to make a few observations upon hiccup.

When hiccup occurs in typhus fever, it is generally owing to a congested state of the mucous membrane, accompanied by flatulent distention of the stomach and bowels. A remarkable case of this sort occurred to Dr. Ireland and myself, in which a the intestinal canal. The bleeding continued many days, the stools being mostly copious, and consisting either altogether of black grumous clots mixed with fluid blood, or else of blood mixed intimately with fecal matter. Sometimes not more than one or two evacuations took place daily, and the debility not being proportioned to the quantity of blood lost, it is more than probable that in such cases the bleeding continued into the bowels in much greater quantity than the blood was evacuated.

In all these cases the hemorrhagic, dicrotous pulse (see page 59) preceded the discharge of blood.

It has been satisfactorily proved by modern investigations, that the dark-coloured matter similar in appearance to coffee-grounds, which is discharged from the bowels in this disease and yellow fever, consists of the coagulum of blood broken down and darkened in tint by the acids of the intestinal canal. I had lately an opportunity of observing a fact strikingly corroborative of this explanation. A young gentleman labouring under very severe fever, with violent headache, was attended by Sir Philip Crampton and I. On the seventh day of his illness, two leeches were applied to the internal surface of his nostrils, and produced a very copious flow of blood, large quantities of which were swallowed by the patient during his sleep. In thirty-six hours after the bleeding had ceased, the nurse-tender became very much alarmed on observing the blackness of the alvine discharges. She told the family it was a very dangerous symptom, and I was sent for in great haste. I need scarcely add that, on seeing the evacuation of so large a quantity of matter resembling coffee-grounds, the true explanation of the occurrence immediately suggested itself, and enabled me to dispel the alarm of my patient's parents.

When blood is swallowed by a person in health, whose digestive organs are vigorous, it never forms anything like coffee-grounds in the large intestines, but is thoroughly digested and absorbed in the superior portion of the alimentary canal.

LECTURE XI.

THE GENERAL TREATMENT OF FEVER.—EMETICS.—PURGATIVES.—BLEEDING.

Having spoken at some length respecting epidemics, one only fact occurs to me in addition to those already detailed. It by no means follows, when fever has a decidedly malignant type, that other acute diseases which prevail at the same time should exhibit a similar tendency; thus measles and scarlatina are often epidemic simultaneously with fever, and yet each of the three may present a different type. In the year 1842 we witnessed a very widely disseminated epidemic of scarlatina, whose character was most malignant and fatal, and yet fever during that period was unusually mild in its form, while measles were rife and of a purely inflammatory character. Here, then, was a year during which fever, without becoming inflammatory, ceased to be *typhus*, scarlatina assumed a typhoid character, and measles prevailed, but of a purely inflammatory type! This statement, for the accuracy of which I can vouch, teaches how difficult it is to explain the causes which give to epidemics their peculiar complexion; indeed, for several years scarlatina had been extremely malignant, and during the same period measles very benign; so that we must not too hastily adopt the hypothesis that some general cause exists capable of simultaneously modifying diseases of different species—an hypothesis which has found many advocates, among the rest Dr. Watson, who says, "Sydenham found that measles of an unusually bad kind prevailed in London in the years 1670 and 1674; the very same years in which small-pox was also remarkably malignant and fatal. This illustrates what I have stated before, viz., that the typhoid tendencies of these and other febrile disorders depend less upon any peculiar virulence in their *exciting* causes, than upon some change previously effected in the human body by the silent and gradual influence of certain *predisposing* causes."*

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* *Lectures on the Practice of Physic*, vol. ii, p. 780, 1st ed.

I have already observed, that it is not my intention to give a systematic account of the practice to be adopted in the treatment of typhus. I have designedly passed over many important points, being unwilling to trouble you with any observations on practical matters in which my opinions coincide with the latest and best authorities. I shall therefore touch very briefly on the subject of emetics in fever, as the rules by which the administration of these remedies are regulated have been laid down with precision by many modern writers.

I am not in the habit of using emetics in fever, except when called in at the very commencement of the disease. Here emetics are of great value, and will often succeed in stopping the fever. There is no way in which you would be more likely to cut short an attack of fever than by the administration of an emetic, if you chance to see the patient when the fever is just beginning. I speak here without any subterfuge, and without grounding my opinions on the results of doubtful or merely suspicious cases. I speak not of cases of bad feverish cold, in which the symptoms, at the commencement, bear a very strong analogy to those which usher in typhus; I speak of cases where the patient gets rigors, followed by the usual symptoms of feverish excitement, after exposure to contagion, and is seen on the evening of seizure.

If I were called to visit a patient who had been attacked with shivering, headache, quickness of pulse, increased temperature of skin, and lassitude, during the prevalence of an epidemic, or after exposure to contagion, and happened to see him a few hours after the attack, I should certainly bleed him, and administer an emetic: and I think he would have a very good chance of escaping the disease. I think the exhibition of emetics an excellent practice in the commencement of fever, but I must observe that the period for their exhibition is very brief. After the lapse of twenty-four or thirty-six hours from the occurrence of the rigor, they will not succeed in cutting short the fever. A few hours make a vast difference in the chances, and after the lapse of twenty-four hours there is, generally speaking, very little hope of extinguishing the disease. At the termination of that period, it has in most cases seized hold of the constitution too firmly to be shaken off by an emetic, even though aided by bleeding; but for the first few hours after seizure, the plan I have mentioned affords you a reasonable hope of being able to put a stop to the mischief at once. Army surgeons, and practitioners who have opportunities of treating incipient disease, are well aware of the truth of these observations. I have myself witnessed many cases in private practice, of medical men and students who had been attacked with symptoms of fever after exposure to contagion, and who escaped by taking an emetic and being bled in proper time.

Let me here read for you a few observations on the use of emetics at the commencement of fever, which appear to me to be very judicious:—

"When the opportunity offers of administering remedies in the first days of fever, an emetic may often be given with advantage, especially where the type of the fever is mild. An emetic clears the stomach of offending matters or sordes, which may be either undigested aliment, bile, thickened and vitiated mucus, or its own thin acid or acrid secretions. Besides which, an emetic has the additional advantage of determining the blood to the surface, and in this way relieving the oppressed state of internal organs. A powerful emetic may sometimes give the system a shock, sufficient to alter the course of the symptoms, and even to cut the fever short. This practice, however, is not without its dangers. In some cases it determines morbid action to the stomach, and renders that organ *irritable* during the whole course of the fever. At other times an emetic brings on local inflammation in some important viscus, on the same principle that it forces out sweat. As a general rule, we are not justified in giving an emetic, unless we have reason to think that the stomach is *foul*, that is, loaded with acrid matters, whether formed within the body, or received into it from without."—*Gregory's Practice of Medicine*, page 121. Sixth Edition.

"*The arrest of fever may be also successfully attempted* during the stage of invasion, or up to the commencement of vascular reaction or excitement; but when once this period has supervened, the fever will run a regular course, although it will often be much shortened by treatment. Fevers, I believe, caused by infection, are very rarely arrested after reaction is established. The means just advised for the formative stage may likewise be tried in that of invasion; but much discrimination is requisite in the choice of means. Camphor, ammonia, and warm diaphoretics and diluents, sometimes with opium when the head is not affected, the warm bath, the vapour or heated air bath, and frictions subsequently, are the most generally appropriate. In robust persons, and where terrestrial emanations have been the chief cause, a warm emetic and active stomachic purgatives may also be exhibited; but they should more rarely be ventured upon in other circumstances, for the reasons just assigned. When there is tenderness at the epigastrium, with other signs of gastric irritation and depression of nervous power, instead of an emetic or cathartic, a large sinapism, or a warm turpentine epithem should be placed upon this region, and over a great part of the abdomen; or, in other cases, upon the inside of the thighs; but neither of these ought to be resorted to if reaction have supervened, nor continued after it has come on."—*Copeland's Medical Dictionary*, vol. i. page 921.

Except at the commencement, then, I am not an advocate for the use of emetics in fever. If they fail in checking the disease, they are apt to be followed by considerable debility of the stomach and general system—states which it would be better to avoid, where the patient has to run through the course of a long and exhausting disease. If called to a case of fever in which you cannot give an emetic, there are two or three other remedial agents you may employ to moderate the feverish excitement, and render the disease milder and more manageable during its progress. One of these is James's powder, with which you may combine blue pill or hydrargyrum cum cretâ, if necessary, giving two or three grains of each every third or fourth hour, according to circumstances. Another remedy, which many are in the habit of using, particularly where the fever is accompanied with symptoms of inflammatory excitement, is a weak solution of tartar emetic. Two grains of tartar emetic may be dissolved in a pint of barley-water, and of this mixture a table-spoonful may be taken every second hour. These are good and useful remedies in the first stages of fever; they moderate the feverish excitement, act gently on the bowels, and produce more or less diaphoresis.

It most commonly happens that the physician is not called to see a case of fever until forty-eight hours, or perhaps three or four days have elapsed, from the period of seizure. In this climate feverish colds are extremely frequent; and as their symptoms bear considerable resemblance to those of incipient fever, and very few are capable of making a distinction between them for some time, a person attacked with fever usually regards it, at the first onset, as the result of cold, and expects to be able to alleviate or remove it in a few days by bathing his feet and taking a warm drink at night, with, perhaps, some opening medicine on the following morning. The usual period, however, at which the feverish cold had been accustomed to decline, passes over without the expected amendment, the patient feels himself weaker and worse, the conviction is brought home to him that his disease is something more than an ordinary cold, and he sends for a physician about the third or fourth day. Now at this period, I believe, you must be content to let the fever run its course; for it has taken root too deep to be expelled by a *coup de main*, and yet many persons seem to think they can still succeed by what they term bold and decided treatment. The mode which they generally adopt is, first, to administer an emetic, and then to have recourse to copious and continued purgation. This leads me to say a few words on the use of purgatives in fever.

The abuse of purgatives, particularly in the first stage of fever, continues, I am sorry to state, even to the present day, a blot on the character of practical medicine. Large doses of calomel and vegetable purgatives, in the form of pill or bolus, followed by draughts composed of infusion of senna, Epsom salts, and electuary of scammony, form the chief part of the treatment in fever with too many practitioners. I know well that this is a mode of proceeding too commonly employed, and I have frequently heard those who adopt it, when questioned as to the remedies they have used, declare, with much self-satisfaction, that the patient's bowels have been well cleared out. This, I believe, is a very common mode of treating fever in the incipient stage; and though there can be no objection to the administration of a purgative, as a cautionary measure, particularly where an accumulation of fecal matter in the bowels is suspected, I must confess that my experience does not authorize me to say that fever can be either checked or mitigated by continued purgation.

If active purgation does not check fever in the commencement, what benefit then can be expected from it? People will tell you that full purging must act beneficially in two ways: by unloading the bowels, and by evacuating the general system. With regard to evacuating the bowels, I think it can be done well and sufficiently by the use of mild aperients. It is seldom necessary to give active purgatives, and we never have occasion to continue their employment from day to day. The bowels, I repeat, can be sufficiently unloaded by the exhibition of mild aperients and enemata, and even these will seldom be required more than once or twice in the commencement, and occasionally during the course of the disease. The second question (in reference to the use of purgatives as general evacuants) is, whether it is prudent or safe to act antiphlogistically on the system through the medium of the intestinal canal, during the first stage of fever? My opinion is, that it is not. I grant that the administration of active purgatives is followed by a copious evacuation of the fluid secretions of the intestinal canal, and that in this way you deplete the system to a very considerable extent. Admitting all this, and, moreover, that depletion is required, still I am of opinion that this is not the best way of effecting it, and shall always give a preference to the action of other remedies. I prefer the action of James's powder, or tartar emetic, or nitrate of potash, or leeches, or, in fact, any remedy which will act with less risk of subsequent mischief.

I have observed that the abuse of active purgatives in the commencement of fever—nay, even the exhibition of cathartics two or three times in the beginning of fever, in persons with irritable bowels, is very apt to induce excitement of the gastro-intestinal mucous surface, giving rise to early and profuse diarrhœa, tympanitis of a bad and unmanageable character, and not unfrequently to disease of the mucous coat of the digestive canal. Great tenderness of the belly, meteorism, and exhausting diarrhœa, are the general consequences of early and continued purgation. In private practice I can generally tell, by examining the patient's belly, whether he has been actively purged in the commencement of the disease or not. I invite you to study the cases that come before you in hospital, with reference to this point; I think you will find in most instances, that the patients who have escaped active purgation before admission will get through the disease with little or no tympanitis. The physician who merely employs mild aperients and enemata—who does not use active purgatives from day to day, as is too often done—will not have his plans of treatment embarrassed by the occurrence of dangerous tympanitis, or obstinate and debilitating diarrhœa; nor will he have the melancholy prospect before him of having an inflammatory affection of the gastro-intestinal mucous membrane to treat, at a period when neither the condition nor the constitution of the patient will bear anything like antiphlogistic measures.

As to purging in general, the idea of curing fever by it is quite absurd. In fever all the secretions are affected, and it would be idle to think of altering and improving all by acting on the bowels. Take the skin, for example. Consider what a departure there is from the normal state; observe the quantities of moisture which exude from it without any apparent cause, or its equally inexplicable dryness. Its odour, its feel, its nervous and vascular conditions, are all more or less altered. Take the lungs, in the next place. There is generally some change in the smell of the patient's breath; there is some change also in the quantity of the pulmonary exhalation; there is an alteration in the rate and mode of respiration; and I have ascertained, by experiment, that a person in fever does not consume as much oxygen, or give out as much carbon, as he would in a state of health. Observe the functions of the brain, or those of the liver or kidneys, and see how much they have departed from the normal state. Every secretion, every function, is more or less deranged, and will remain so as long as the fever lasts. You have no right to think that you will be able to restore the healthy state of the stomach and bowels any more than that of any other organ. The secretions of the lungs, liver, pancreas, kidney, stomach, and skin are all deranged, or more or less suppressed, and will not be restored to a healthy state until a crisis comes on, or the disease begins to decline.

As long as the belly is soft and fallen, and where the bowels have been sufficiently opened in the commencement of the disease, I do not feel the least anxiety if the patient remains without having a stool for two or three days. I have, on some occasions in private practice, been induced to consent to the exhibition of a purgative where I did not think it required; and have seldom done so without regretting it afterwards. The patient has been going on well, the belly soft and fallen, no tenderness present, and no distinct evidence of fecal accumulation.

All this I have pointed out to the practitioners in attendance with me, but to no purpose. They would generally observe in reply, "Oh! this may be all true; but you see the patient has had no stool for the last thirty-six hours, and it would be quite wrong to let him go on in this way any longer." Indeed, you will frequently meet with cases in which you should exercise much caution in the administration even of enemata. An illustration of this remark occurred to me lately in practice. In a case of fever in which the patient's friends were importunate as to the necessity of opening the bowels, the ordinary purgative injection was prescribed. It proved too active, and produced much irritation of the bowels, giving rise to an increased secretion of gas into the intestines, and a considerable degree of temporary tympanitis.

You will be guided, therefore, in the administration of purgatives, not by the rule of those who are dissatisfied with less than two or three motions in the day, but by the circumstances and exigencies of the case; and you will be cautious in giving purgatives, except where you have good reasons to conclude that there is an accumulation of feces. In this way you will avoid tympanitis, diarrhœa, and inflammatory affections of the bowels; symptoms which always give great annoyance to a practitioner, and tend greatly to embarrass his practice in the treatment of all fevers of a typhoid character.

So far concerning the administration of purgatives as a cure for fever, or as a means of diminishing its violence. You perceive that I think their employment more than questionable, and in this particular am consequently at issue with Hamilton, and a great number of writers. There are, however, circumstances which may arise during the course of typhus, and may require a free use of purgative medicines; we are then forced to have recourse to purgatives, not in the hope of curing the fever itself, but for the purpose of removing or alleviating certain superadded symptoms.

It may be well to mention some of the chief of these symptoms. One of the most common is determination of blood to the head, producing delirium, headache, &c. In many examples of this nature, occurring at an early period of typhus, purgatives of a very active nature are amongst our most efficacious remedies. Nay, even in the advanced stages of fever, delirium and determination to the head are seldom relieved by tartar emetic, unless it produces very copious, yellow, watery stools. Many patients become uneasy and restless at night in the latter periods of fever, in consequence of insufficient evacuations from the bowels; whenever, therefore, restlessness or sleeplessness supervene unexpectedly, and that the bowels are confined, the occurrence of these symptoms calls for aperients, even though the belly be not very full and tumid. Preternatural fulness of the belly and tympanitis often demand purgatives at any period of the disease.

In some cases, when a troublesome diarrhœa has yielded to astringents, a very obstinate and long-continued state of constipation comes on, apparently connected with impaired muscular power of the intestinal tube. At first the confinement of the bowels produces no uneasiness on the part of the medical attendant, inasmuch as it is unattended by any fulness or tension of the abdomen, and the patient may, in other respects, appear to be doing well. After some days, however, it is judged prudent to excite alvine evacuations, which is attempted cautiously, for the practitioner bears in mind the violence of the previous diarrhœa. He therefore chooses mild purgatives at first, and next day, finding them ineffectual, he ventures on the exhibition of more active medicines, and orders a frequent repetition of injections. Even these steps fail, and constipation continues for several days after the efforts to remove it have been commenced. This is a juncture full of difficulty. In such cases, much caution must be used in employing active cathartics, and great care should be taken to remove any hardened feces which may be present in the rectum or sigmoid flexure of the colon. This must be done partly by the finger, or by means of an appropriate scoop, as, for instance, a marrow spoon, and by injections of soap and water. When no such mechanical obstructions exist, to account for the failure of the cathartics, we must proceed cautiously, and not rashly accumulate medicines of this description in the stomach and bowels of the patient.

Very active purgatives, though they fail to stimulate the paralysed bowels so as to evacuate their contents, may yet irritate their intestinal mucous membrane, and cause destructive inflammation. For this reason, where moderate doses of colocynth, gamboge, jalap, scammony, rhubarb, &c., have failed, they must not be repeated; neither, except in desperate cases, ought we to administer croton oil internally. The neutral salts, senna, magnesia, and, above all, castor oil, given combined with oil of turpentine, or uncombined and very frequently repeated, must be our chief internal medicines. In some cases, the compound decoction of aloes, with small doses of sulphate of magnesia, will succeed in exciting the paralysed bowels to action, where other and more powerful purgatives have failed. Injections should be perseveringly repeated, and varied both in quality and quantity; and they should be always thrown as far as possible into the bowel, by means of a flexible tube and Read's syringe. When they are retained, and excite swelling of the belly, as too frequently happens in these cases, we must desist from their use.

This obstinate state of constipation may be supposed to depend on a degree of paralysis of the bowels; for usually in such cases an evident paralysis affects the bladder, causing retention, or its sphincters, giving rise to an involuntary dribbling of urine.

On the subject of bleeding in fever I have but very few remarks to offer. In the first place, with respect to the power which venesection possesses of checking fever, it may be observed, that there can be no doubt that it has frequently been found capable of effecting this purpose, particularly where it has been properly employed, and in conjunction with other means. I speak here with reference to cases in which bleeding has been used under favourable circumstances, and very soon after seizure—as in students, medical practitioners, hospital attendants, soldiers, and seamen. In such persons, and others where circumstances have been equally favourable, there is no doubt that venesection has frequently succeeded in cutting short fever; and if called to a case of typhus within the first ten or twelve hours after seizure, I should have no hesitation in having recourse at once to venesection, followed by an emetic; and my own experience convinces me that I should afford my patient a very good chance of escaping the disease. I have on several occasions succeeded in arresting the progress of fever by these means; and the records of naval and military practice furnish many proofs in corroboration of my statements. I have also the authority of Dr. Cheyne (whose experience on every point connected with fever was immense) in favour of the efficacy of bleeding in commencing fever, as a mode of treatment which has frequently proved successful in his hands. But it is only in the very commencement, and during the stage of rigor, that you can hope to derive any advantage from venesection in cutting short an attack of fever. I do not mean to say that you have in typhus, as in intermittent fever, distinct rigors, lasting each for half an hour, or even longer. By the stage of rigor in typhus I mean to designate the period of formation, during which the patient complains of recurrent chills, although his skin feels hot to the touch when examined by another person. This stage lasts generally from twelve to twenty-four, and in a few cases to thirty-six hours; and it is only during this stage that you have a chance of extinguishing the fever at once, by the abstraction of blood from the system.

You may also have recourse to venesection within the first day or two, for the purpose, not of arresting fever at once, but of lowering inordinate vascular action in persons of a robust habit, and where the fever sets in with violent headache, great heat of skin, and a firm bounding pulse. We do not, however, at present meet with many such cases, nor are we often called in at a period when venesection might be advantageously practised. The physician seldom sees a case of fever until the third or fourth day, and then it is too late to think of general depletion by the lancet. This explains why venesection is so seldom employed in typhus in our hospitals.

Moreover, in entering on the treatment of any case of fever, you should bear in mind the nature of the prevailing epidemic, and be careful how you proceed with respect to bleeding; and if you take away blood, do not go so far as you would if treating a case of fever under different circumstances, and of a genuine inflammatory character. I know that many persons have asserted that you can bleed in all cases of fever, no matter what the state of debility may be; because this, they say, is only apparent, and depends upon congestion and oppression of vascular action. I do not know how far this doctrine may be applicable to former epidemics, but, in the recent epidemics of fever we have had, it certainly does not hold good; and no man in his senses would think of adopting it as a guide for his practice. I have seen some of the most intense, dangerous, and protracted cases of fever, commence without any appreciable increase of vascular action, with a soft slow pulse, a cool skin, no appearance of congestion of any internal organ, in fact without any symptom which would, even in the youngest and most robust habits, call for the use of the lancet.

Increased vascular action, and this, you should always bear in mind, is not in itself a proof of an inflammatory diathesis in fever, but rather one of a set of symptoms produced by the same morbid cause. The heat of skin and rapidity of pulse are, just like the debility, products of the same morbid cause, and not the results of inflammation. You should also recollect that in fever, as well as in other diseases in which the nervous system is greatly deranged, the pulse is not unfrequently a very deceptive guide. In many cases of fever, where the patient happens to be of an irritable habit, the pulse exhibits a degree of thrill and apparent hardness, which might lead an inexperienced or inobservant practitioner into serious errors. I do not mean to say that an inexperienced finger will not be able to distinguish a pulse of this kind from one of genuine hardness, but I know that many persons have been misled by it, and I warn you against the danger.

Again, never use the lancet when there is any, even the slightest appearance of maculæ, no matter how intense the headache, heat of skin, or signs of general vascular action may be. I have seen some cases in which the lancet was used during the presence of maculæ, and I have seen its employment followed by the most lamentable consequences. You should, therefore, never omit to examine the skin, for circumstances might occur which would authorise a moderate use of the lancet, provided there was no sign of maculæ present. Formerly, persons were very much in the habit of employing arteriometry when the headache and delirium were violent, regardless of the period or stage of fever; and nothing was more common than to see a physician ordering the temporal artery to be opened on the eighth, ninth, or even tenth day. This was very much the practice during the time when the doctrine of typhus being the result of inflammation of the brain prevailed in this country and England, and a very unsuccessful practice it was. You perceive we seldom have recourse to arteriotomy here; it may be occasionally necessary, and when it is, we employ it; but as a general practice it does not appear entitled to any merit, nor can we give it our recommendation.

The examples which you have seen in hospital show you that local inflammation arises, generally speaking, at a period when general bleeding is no longer admissible. I shall speak hereafter of the mode in which leeches are to be applied to the head, with the view of relieving headache and cerebral congestion; it is not necessary that I should say anything respecting their application to the epigastrium, or abdomen, for the relief of gastro-intestinal symptoms in the beginning of fever, as there is very little chance of your doing any mischief, even by the free use of leeches, at this period: it only remains for me to make a few remarks on the use of leeches and cupping-glasses, in the more advanced stages of the disease.

Well; your patient, suppose about the ninth or tenth day, gets pain in the side, cough, and increased frequency of respiration, and, on examination, you find sufficient evidence of the existence of pneumonia. Or he complains of abdominal symptoms, and you have strong reason to think that hepatitis or enteritis is present. Here you will have recourse to leeches or cupping, according to the circumstances of the case. An attack of pneumonia, coming on in fever, frequently acts as a stimulus to the economy; the collapse of fever disappears more or less, and the pulse becomes more firm and resisting. This is a fortunate occurrence, for under such circumstances the patient is better able to bear depletion, and you may proceed at once to apply cupping-glasses or leeches to his chest, regulating the quantity of blood you abstract not only with reference to his present symptoms, but also to his future condition. But it sometimes happens that pneumonia occurs at a later period of the disease, and when you cannot use cupping-glasses, or even leeches, to any great extent. In such cases (and the same remark will apply to enteritis, or any other inflammation occurring in the advanced stage of fever), you should leech with great caution. Begin with four or six at a time, and when they drop off, cover the leech bites with a cupping-glass. In this way you will know pretty nearly the exact quantity of blood which the patient has lost, and you can arrest it with less difficulty afterwards. You can then have recourse to calomel and opium, or tartar emetic, according to circumstances. Leech as far as you can, and then have recourse to immediate blistering, and such other means as the exigencies of the case may demand.

You may leech, then, freely, and without any particular caution, in the commencement of fever, whether it be for cerebral or for thoracic or abdominal symptoms; but, as the fever advances, you must exercise more discrimination and care, both as to the number of leeches you apply, and the time you allow them to bleed. In applying leeches to the head, I would advise you not to put them on both temples or behind both ears at once, as this is awkward, and prevents the patient from lying on either side. You may also, in cases of cerebral irritation, apply them to the nostrils or *septum narium;* in this way you will be able to get away a large quantity of blood by means of very few leeches, for one or two at a time will be sufficient. In leeching the chest and abdomen, in particular, I advise you never to have recourse to fomentations with the view of getting more blood from the leech-bites. Fomentations are too often a source of fresh mischief in cases of this kind, leading to exposure of the patient to cold, and to the annoyance of having his linen and bedding kept wet for hours together. Always give directions to have cupping-glasses or hot dry flannel cloths applied as soon as the leeches drop off, and you will have less difficulty in arresting its flow afterwards, a point of some importance in cases where the loss of even a trifling quantity of blood is often of great moment, and likely to have a very powerful effect on the state of the patient.

LECTURE XII.

THE USE AND EMPLOYMENT OF BLISTERS IN FEVER.

BLISTERS are employed in a variety of diseases, but are followed by very different physiological effects, and capable of serving very different purposes, according to their mode of application. In fever they are generally employed either as stimulants, or as evacuants and derivatives. As stimulants, they may be used with the intention of rousing the depressed energies of the system in general, by their action on the nervous and circulating systems, or of stimulating the torpid functions of some particular part or organ. With this object in view they are applied as flying blisters—that is to say, for a space of time not exceeding two or three hours, and solely with the intention of producing a stimulant effect. You have seen some cases of fever in our wards, in which the powers of life were greatly depressed, the extremities cool, the action of the heart feeble, the pulse weak, respiration short and imperfectly performed, and a tendency to faintness and sinking; and you have observed that in such cases we derived great benefit from the application of flying blisters over the region of the heart, the epigastrium, chest, and inside of the legs and thighs. We applied our blisters in these situations, left them on for three or four hours, and then removed them; and you have seen them, when employed in this way, succeed in rousing the vital energies, the depressed action of the heart and capillary system, and the flagging state of the respiratory action, as shown by the increased strength of the pulse, the more general diffusion of heat, and the renewed play of the various functions.

In such cases, where the stimulant effect alone is required, it would be wrong to leave the blisters on longer than two or three hours; it will be quite sufficient if they prove merely rubefacient, or, at most, vesicate so slightly as to give to the blistered surface the appearance of a miliary eruption. Here you have all the stimulant effects of blistering, but not followed by their debilitating consequences. You are aware that blisters applied in the ordinary way have a twofold effect; they first rouse, and then depress; acting primarily as stimulants, and secondarily as evacuants. They first act as stimulants, producing pain, heat, and redness of the part; after a few hours these symptoms diminish, and are followed by an effusion of serum—in fact, a quantity of white blood is abstracted from the cutaneous capillaries, and in this way an evacuation is produced, calculated to diminish any accidental congestion in neighbouring parts. The capillaries, by means of their increased action, draw a quantity of white blood to the part; and, in saying this, I think I am only using a perfectly physiological expression, for the quantity of circulating fluid in any part of the body must depend on the vital action of the capillary vessels of that part. It is to the peculiar state of the capillary vessels, as I have proved in a previous lecture, that the quantity of blood in any part is to be referred, and not to the force or frequency of the heart's action. It is by means of changes produced in them that the phenomena of active congestion and inflammation are produced; the capillaries of the affected part enlarge, increase in number, and multiply; and those which were invisible become visible. These phenomena have been falsely attributed by Hastings and others to debility and impaired action of the capillaries.

Blisters, then, produce first increased action of a part, and afterwards act as evacuants. They also stimulate the system generally; but if left on until full vesication is produced, they act as evacuants and depletives, and lower the general tone of the economy. I have frequently observed this succession of events in chronic cases, in which it was found necessary to blister repeatedly during the course of the disease. The patients generally told me that they felt better and lighter on the day on which the blister was applied, but on the next day they usually felt weaker and more depressed; and this state sometimes lasted more than a single day. You may, therefore, apply blisters as excitants and stimulants; yet there are many persons who seem to forget this distinction. If, in a case of inflammation occurring in a low state of the system, you propose to apply a certain number of leeches over the inflamed organ, they say, no; but they have no hesitation in applying a large blister, leaving it on until it produces full vesication, and thus abstracting a considerable portion of white blood from the system.

You will not expect me to lay down any general rules for the use and application of blisters in fevers; you will find all these matters sufficiently explained in your books and manuals. I am not giving anything like a regular outline of the treatment of fever; in fact, I pass *per saltum* from one point to another, without any attention to order or method. You can read methodical treatises, and then compare them with such detached observations as I shall make. And here allow me to make some cursory remarks on that peculiar state of the brain which we most commonly observe in the middle stage of typhus, and in which blisters form one of our most efficient, and in some instances our only mode of relief. In many of the cases of typhus which come under our observation in hospital, we frequently meet with a train of symptoms strongly calculated to perplex and puzzle, and which should seldom exist in fever regularly treated; these are chiefly cases which are admitted in the middle or latter stage of the disease, and at a period when the patient's state of intellect is such as to preclude the hope of obtaining any satisfactory information from a personal examination.

A man in the lowest class of life, and at a distance from medical aid, is attacked with fever; for the first eight or ten days he is either improperly treated or altogether neglected, and in this state symptoms arise and superinduce others, causing the most unfavourable complications, and rendering the cure difficult, if not impossible. Now, of all the symptoms which occur in cases of fever, where the state of the principal organs has been neglected, there are none more formidable or more fatal than the cerebral; nor is there any local affection in fever, in which the value of prevention is so unequivocal and decided. What I wish to impress upon you is, that you should always anticipate the cerebral symptoms in fever. Never allow the cerebral symptoms to explode—watch the first scintillæ of cerebral excitement—repress the commencing mischief, and do not permit your patient to be overtaken by formidable inflammation of the brain.

Every writer will tell you that when the patient's face is flushed, his eyes suffused, and when he complains of headache and intolerance of light, you should leech and blister his head, hand. In this case, the patient is almost continually sleeping. When you enter his chamber in the morning, and ask how he does, his attendant generally tells you that he has passed the night most favourably, and that he has slept without almost ever waking since your visit on the preceding afternoon. If he awakens to take drink, he quickly drops asleep again, and when you rouse him he looks rather heavy; there is some slight suffusion of the tunica adnata, and some appreciable congestion about the external parts of the face and head. Persons in this state, though apparently doing well, and even where they have been properly treated in the beginning, about the ninth or tenth day begin to rave, and exhibit undoubted proofs of congestion and excitement of the brain.

Now, in all cases of this description, be on your guard, and do not allow symptoms of dangerous import to steal on you. Here you will derive great benefit from the use of blisters. I was lately called to a very remarkable case of this kind, at some distance from Dublin. The patient slept almost constantly, and complained of no headache or heat of scalp. From an attentive examination of the case, however, I was led to predict the approach of cerebral symptoms. Observe, this was a case of spotted fever; and in this form of fever you can predict the occurrence of such symptoms with a greater degree of confidence. The patient's pulse was 96, his tongue presenting nothing worthy of remark, his behaviour and speech rational, and his sleep almost constant. Recollecting, however, the period of the fever, and observing carefully the condition of the cerebral functions, I had his head shaved and blistered. Notwithstanding this precaution, his cerebral symptoms had proceeded so far that he subsequently got a slight attack of paralysis of the face and tongue, accompanied by a fixed state of the pupils, which would neither contract nor dilate. After having blistered his head extensively, I gave him the tartar emetic solution, to the amount of one-eighth of a grain every second hour. The measures were completely successful in removing the cerebral symptoms, and I have no doubt that the active precautions which had been taken were the means of saving his life.

There is one symptom connected with cerebral excitement in fever which is well worthy of your notice, as its existence is often sufficient of itself to give timely intimation of the approach of irritation or inflammation of the brain. This is the state of the respiratory function. In fever, the breathing will often announce the approach of cerebral symptoms for days before their actual occurrence. When, in cases of typhus, you find the patient's breathing permanently irregular, and interrupted by frequent sighing—when it goes on for one or two minutes at one rate, and then for a quarter or half a minute at another rate, you may rely upon it that sooner or later an affection of the brain will make its appearance. You will frequently observe the same kind of breathing preceding attacks of apoplexy and paralysis, and indeed it was the occurrence of this symptom, in these and other cases in which the functions of the brain were deranged, that first drew my attention to this kind of breathing. The first time it engaged my attention was in a remarkable case of an apoplectic nature, which I sat up a whole night to watch. On recollection I found that I had frequently observed an analogous state of the respiratory function in fever on several occasions, although its connexion with excitement of the brain had not struck me before. I speak here of irregularity of breathing, independent of any pectoral affection. But when the patient breathes in a permanently irregular manner, at one time at a certain rate, and at another at a different rate,—when his respiration is suspicious and heaving, without any disease of the chest or great debility,—you will have some grounds to suspect the existence of cerebral derangement. I am in the habit of calling this kind of breathing *cerebral respiration*, because my experience has told me that it is almost invariably connected with oppression and congestion of the brain.

To recapitulate:—When you find a patient in fever lying constantly awake, or when, on the contrary, you find him continually slumbering,—when there is a certain quickness of manner and irritability,—and when the cerebral respiration has been noticed for some time, without any concurrent debility or pulmonary disease,—under such circumstances, you may, in cases of maculated typhus, predict the approach of cerebral symptoms; and the period about which they generally manifest themselves is the eighth, ninth, or tenth day. Now, in cases of this description,—if you have previously used leeches and antiphlogistics to a sufficient extent,—your best plan will be to shave and blister the whole scalp.

Dr. Little of Belfast, and Mr. Kirby of this city, have fallen into the same train of ideas, and employ blisters at a very early period of the disease, with the view of combating cerebral excitement. In a recent instance, in private practice, I think I saved the life of a young gentleman in Harcourt Street, by extensive blistering of the scalp on the fourth day of fever. We were not accustomed to blister at this early period of fever. Formerly it was the practice to bleed and apply leeches for several days together, and never to have recourse to blistering until towards the latter stage of the disease. In common inflammation, or in arachnitis, we do not blister until we have carried depletion by the lancet, leeches, and purgatives, as far as the patient's strength will allow. But this is not the case in fever; the cerebral congestion and irritation, or inflammation, (call it which you will,) which accompanies typhus, differs essentially from ordinary arachnitis or encephalitis, and requires very often a treatment strikingly different.

One physiological fact connected with sleep may be noticed here. It has been stated by Mr. Mayo, that the pupils are contracted during sleep. This is in itself a very curious fact, and I was anxious to verify it. Now we had an excellent opportunity yesterday morning of trying what the state of the pupil was, in two patients who lay soundly sleeping in the fever ward. We came up softly to them as they lay on their backs, and in a most favourable situation for observation, just opposite one of the windows; and having opened the eyelids, found that the pupil was actually contracted to the size of a pin-hole. It remained in this state for a while, and then expanded, when they awakened. This is a very curious fact, and appears to be a very beautiful instance of the protective care of nature. To protect the eye while we sleep, nature, as it were, draws the curtain, and thus defends the delicate organ from any accidental dazzling, at a period when consciousness slumbers and is off its guard.

I have hitherto spoken of blistering in fever chiefly as a powerful revulsive remedy in the treatment of cerebral congestion; let us now treat of its employment with other objects in view. In the first place, as has been already explained, blisters may be used as most energetic stimulants in cases where the powers of life flag, and threaten a sudden cessation. Occasionally, in fever, you will find the vital tone reduced to a very low pitch, the heart uncertain in its action, the pulse irregular, the respiration feeble, the skin cool, and the patient so weak that he cannot be lifted up, or even turned in bed, without having a tendency to faint. Here we have to superadd to the ordinary treatment of fever the prompt exhibition of remedies calculated to meet such emergencies, and in addition to internal stimulants we have recourse to powerful stimulation of the cutaneous surface, by what are termed flying blisters. One of the best remedies in such cases is a large blister applied over the region of the heart, to be left on for two or three hours, or until the vascular action of the skin is sufficiently excited. When the patient appears to labour not only under sudden weakness of the heart, but also of the capillary and nervous systems, as shown by coldness of the extremities and sinking of the pulse, it will be necessary to apply flying blisters not only over the region of the heart, but also over various parts of the chest, the epigastrium, and the inside of the legs and thighs. You will find this plan of treatment frequently succeed in cases which have a very unpromising aspect. I have now witnessed many instances of this description, in which, from cold, neglect, or debilitating treatment, the patients appeared moribund, with lividity of the extremities, hippocratic face, cold skin, and failing pulse; and I have seen them saved, as it were miraculously, by the use of carbonate of ammonia, musk, and wine, and the application of warm fomentations to the limbs, followed by a succession of flying blisters.

Let us take as an illustration the case of Christopher Nolan, which I trust you have all watched with attention. When this man came into the hospital his condition appeared to be completely desperate, he has, however, not only rallied, but is now convalescing rapidly. It is unnecessary for me to enter into a detail of his case, as I trust you have all observed it through its different stages; I shall only remark, that on his admission he was labouring under fever of the worst character, his body was covered with maculæ, he lay constantly on his back, and had low muttering delirium, was unable or unwilling to answer questions, his breathing was oppressed, his pulse rapid, small, and failing, the powers of life awfully prostrated,—in fact, he was in a state of apparently threatening dissolution.

My first object was to rouse the sinking powers of the system, and with that view I adopted the following treatment. He was put into a comfortable bed, and heat was restored to the surface by diligently rubbing his trunk and limbs with warm flannel. I next ordered a succession of flying blisters to the neck, chest, and abdomen. I may observe here, that his chest was heaving, there was a general wheezing audible over the whole surface, and he had that peculiar livid expression of countenance and dusky hue of skin, which indicate an imperfect aëration of the blood. With the view of stimulating the oppressed action of the respiratory nerves, I had two blisters applied, one on each side of the neck, above the clavicle; after remaining on for two hours these were removed, and two more applied over the supra-mammary region, then over the heart and right side of the chest, and lastly over the epigastrium. In addition to this, he was ordered to have some wine and chicken broth, and a stimulant draught was prescribed, to be taken regularly every second hour until symptoms of reaction began to appear.

In employing blisters in this case, my object was to stimulate powerfully and in rapid succession the integuments of the neck, chest, and abdomen. This practice has in such cases been attended with very marked results, and is sometimes proved extremely valuable. Its efficacy seems to depend not on the discharge of serous fluid, or on any revulsive action of the blisters, but on the powerful stimulus applied to an extensive cutaneous surface.

Blisters applied successively to the chest, [illegible] are not only valuable in croup, but also in other diseases, and that under circumstances in which little benefit could be expected. The same effects may be produced by rubbing the whole body with some [illegible] ointment; but, from the pain and inflammation it produces, this proceeding is seldom adopted. I have, however, occasionally employed it, and in two [illegible] with the most fortunate results. [illegible] had lost two children from hydrocephalus. [illegible] [illegible] and advised the child's father to have the whole of the blistered scalp well rubbed with tartar emetic ointment. The boy recovered completely. I derived also a very striking advantage from the use of the same remedy in a very remarkable epidemic which attacked a family in the neighbourhood of Rathmines, and which was witnessed throughout its whole course by Dr. Burke and myself. One of the family, a young lady, was attacked with symptoms of fever, accompanied by a pain in the back of the head, and stiffness of the neck. After a few days, symptoms of inflammation of the cerebellum and upper part of the spinal cord became developed. About the seventh day she got strabismus, and soon afterwards was attacked with convulsions: the pupil became permanently dilated, and she was quite blind. I was called to see her at this period, and found her almost in a state of insensibility, with involuntary discharge of urine and feces, cold extremities, and irregular pulse. Thinking that nothing could be done for her, I was about to leave the room, when I asked the nurse could she swallow? She replied she could, and immediately proceeded to offer the young lady some drink, which she swallowed without any difficulty. This at once arrested my attention. I said to myself, if this patient can swallow, she must be still conscious, and while she is so, there is a chance of saving her. I ordered the whole of the scalp, which had been previously blistered, to be rubbed with tartar emetic ointment; violent inflammation ensued, and she recovered completely. But the curious part of the case is this: —her brother and sister were attacked in exactly the same way, a few days afterwards, although less formidably, and were cured by the same treatment. Shortly afterwards two of the servants got pain in the back of the head and stiffness of neck, followed by signs of an inflammatory affection of the cerebellum and spinal cord. They were treated in the same way, and recovered.

What could be the cause of this peculiar fever, manifesting itself in exactly the same way in all the individuals of the family who were attacked? I endeavoured to arrive at the cause, but could not; and I merely state the facts, without wishing to attempt anything like an explanation. But the history of this extraordinary form of disease is exactly as I have told you.

The next use to which we apply blisters is in the treatment of those pulmonary affections which arise during the course of typhus. From what you have seen of the present epidemic, you must be convinced that bronchitis is one of its most frequent complications, and that few persons pass through fever without having some affection of the bronchial mucous membrane. You are also aware, that when bronchitis attacks the more minute ramifications of the bronchial tubes, it is very apt to produce congestion and engorgement of the lung. We meet with pneumonia much less frequently in fever, but it is occasionally observed, and requires the most prompt and decided treatment. In pneumonia, as well as in congestion of the lungs accompanied by inflammation of the smaller bronchial tubes, blisters afford us a most valuable adjunct to the other means which we employ, and admit of being used in cases where no other mode of depletion could be safely borne.

The affections of the lung in fever are of no small importance, and the stethoscope has not conferred a greater benefit on practical medicine, than by indicating, in diseases of the chest, not merely the existence of disease, but also its locality, extent, and precise nature. It points out to us the portion of the chest in which the bronchial tubes are chiefly engaged, and informs us with certainty when the affection of the smaller tubes has given rise to pulmonary engorgement. The experienced stethoscopist will in such cases be aware of the exact site and nature of the affection, where the mere symptomatic practitioner would be unable to acquire anything more than a loose and undefined notion of pulmonary disease. The latter employs his depleting means at random, and frequently abstracts a large quantity of blood with little benefit to his patient; the former, aware of the precise situation and extent of the disease, applies his leeches or cupping-glasses immediately over the engorged or inflamed portion of the lung, and relieves his patient at the expense of a comparatively small loss of blood. The same observation will apply with equal force to the use and application of blisters.

A good and accurate knowledge of the various stethoscopic phenomena is besides of so much more value in the treatment of fever, as at certain seasons of the year almost every case of fever will be complicated with pulmonary derangement; and it may happen, during the course of an epidemic, that the lungs may be the organs which are chiefly engaged. Although cerebral any other part not exposed to pressure or friction. As soon as the blister rises, apply over it a piece of lint smeared with spermaceti ointment, which can be renewed as occasion requires, and leave the rest to nature. I was forcibly struck some time since with the difference of result between this and the ordinary practice, in the case of a young gentleman residing in Camden Street, who had a severe attack of bronchitis towards the termination of fever. A blister had been applied to his chest in the morning, and another in the middle of the day. The first had been opened freely, and dressed in the usual way; but the other, which had risen about the time I was called in, was left untouched at my request. The one which had been opened caused such a degree of irritation and restlessness, that it was found necessary to give him an opiate every night; the other gave little or no inconvenience, and healed up much sooner. A still better method is that recently proposed by Dr. Douglas Maclagan of Edinburgh—dressing the blistered surface with cotton. The blister is left on for five or six hours, according to circumstances, a poultice then applied for two hours, and, the raised cuticle having been removed with a pair of scissors, the raw surface is covered with a thick layer of French wadding. It heals completely in about twenty-four hours, but is so little painful after twelve hours that percussion and auscultation may be performed over the part—of course without disturbing the cotton, a matter of much importance in pulmonary affections.

If I have done nothing else, I think I deserve some merit for being the first to reprobate the practice of keeping on blisters for twelve, eighteen, and twenty-four hours, and for having shown, by numerous experiments, that a much shorter period of time was required to ensure the full effect of these remedies. When I commenced the practice of medicine, blistering was looked upon by most sick persons as one of the severest trials of their patience, and the agony which it caused in some irritable habits was almost insupportable. Blisters were left on for twelve, eighteen, and even twenty-four hours, and when at length they were removed, the whole epidermis of the blistered part came or was torn away, leaving behind a raw irritable surface, from which large quantities of serum and pus were discharged for several days, to the great distress and inconvenience of the patient; and the [illegible], [illegible] the practitioners of that time generally [illegible] the excoriated surface with some sharp stimulant ointment, so that the blistered surface most commonly resembled that of a severe burn.

Ask those who are our seniors in practice, and they will tell you what blistering was some thirty or forty years ago. They first produced excessive irritation of the skin, by leaving the blisters on too long; they then irritated the denuded surface with stimulating ointments, and in this way brought on extensive sores of a bad character, which remained long after the disease for which the blisters were applied had disappeared, and which formed, in fact, a new ailment, requiring new medicines and additional attendance. If you look over Mr. Moore's account of the principal remedies employed in the practice of Dublin physicians about the period I allude to, published in the tenth volume of the *Dublin Journal of Medical Science*, you will find that nothing was more common then than the application of stimulant, and, as they were termed, digestive ointments, to blistered surfaces. I was among the first who assailed this barbarous treatment, and showed that all the good effects of blisters might be secured by leaving them on for a much shorter space of time. I proved by numerous experiments, that in many cases it was not necessary to leave them on more than four or five hours in the adult,* and that they might then be removed and the blistered part dressed with spermaceti ointment. In addition to this, you entirely avoid the irritating effects which blisters are known to produce on the urinary organs. You will very rarely meet with dysuria, or hematuria, where the blister has been left on for the spaces of time I have mentioned.

Blistering is then to be used with the restrictions I have mentioned, and you will find it a most valuable aid in the treatment of fever and its complications. It may be employed either as a derivative and revulsive, or you may have recourse to flying blisters over the various parts of the body, in certain forms of fever, where there is marked and sudden depression of the powers of life.

Speaking of depression of the powers of life, reminds me of a curious incident which occurred some time ago in my practice,

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* Of course blisters applied to the scalp must be excepted. They require at least twelve hours. In old persons generally the skin is much less vascular than during youth and middle age; and consequently, in the old, blisters require a much longer time to produce the desired effect.

[illegible]

[illegible] in disease. [illegible] making [illegible] diagnosis [illegible] direct evidence of [illegible] is of the greatest value and importance in the treatment of disease.

LECTURE XIII.

WARM FOMENTATIONS TO THE HEAD IN FEVER.—USE OF MERCURY.—SUBSULTUS TENDINUM.—CEREBRAL SYMPTOMS.

I HAVE already laid before you my views as to the use of general and local bleeding in fever, and pointed out the circumstances under which they might be employed. In treating of general bleeding, I stated that we used it at the commencement of fever, with a view of checking the disease altogether, or of rendering it milder and less dangerous, by moderating excessive inflammatory action, and controlling cerebral excitement. I have also spoken of the use of leeches and blisters, and it only remains for me to say a few words respecting the application of cold to the head as a means of moderating or removing symptoms of cerebral excitement.

In Dr. Southwood Smith's Treatise on Fever, you will find many cases and arguments to show that where headache and delirium are present, and where the lancet is inadmissible, if you place the patient in a warm bath, and direct a forcible small stream of very cold water on his head, he soon becomes more calm, experiences great relief of his headache, and is frequently brought back to bed quite free from cerebral symptoms. The burning heat of the skin is quickly replaced by a sensation of coolness, or even cold, the flushing of the face disappears, the delirium vanishes, and a favourable crisis is often produced. Indeed, the effects of this remedy are extremely remarkable, and I have no doubt that many of the cases in which I have employed tartar emetic with such signal advantage would derive equal benefit from this mode of treatment.

The cold affusion, as recommended by Dr. Smith, and practised at the Charité Krankenhaus at Berlin, is most certainly an excellent and energetic remedy, and I regret that we have not apparatus in this hospital for applying it; but I fear its utility must be, at least for some time, limited to public institutions, and that it cannot be employed to any extent in private practice. There is a good deal of prejudice against applications of the kind in this country. At the time that cold affusions were used in the treatment of scarlatina, much mischief was done by their indiscriminate employment, and this added to the general feeling of dislike towards them. At all events, cold affusion is a remedy which requires an apparatus seldom at the command of the physician in private families, and, indeed, I think that in most cases we may do very well without it.

You are all aware that, in cases of determination to the head, the common practice is to shave the scalp, and apply cold lotions. In my lectures I have repeatedly pointed out the imperfect and even hurtful mode in which this remedy is ordinarily applied, and endeavoured to show that it is calculated rather to increase than diminish the heat of the integuments. Cold lotions act as a powerful refrigerant, if constantly repeated, so as to keep the part below the standard temperature of the body. But this is seldom or never done. The nurse applies the lotion, and then, perhaps, drops asleep, or occupies herself with some other business, until at last she is attracted by the vapour arising from the patient's head, and then she renews the application. I need not say, that in this way all the good effects of cold, as a refrigerant, are entirely lost, and that a degree of reaction is produced which must altogether mar and nullify its application. I have, therefore, given up, except in very few cases, the practice of applying cold lotions, and give a preference to the use of warm fomentations of equal parts of vinegar and hot water, applied to the temples and shaven scalp, and frequently repeated. I am quite sure we employ warm applications for the relief of headache and cerebral symptoms much less frequently than we ought. You are aware that surgeons are in the habit of treating some local inflammations with warm, and others with cold applications, and that the rules laid down for distinguishing the cases in which cold, and those in which warm fomentations should be used, are deficient in precision, and that most commonly the practitioner has to refer to his own individual experience for the guidance and determination of his choice. So it is, also, with respect to the use of fomentations to relieve the pain and congestion of internal parts, among which I include determination to the head in fever, accompanied by intense headache, restlessness, and delirium. In some cases of this description, cold applications will give ease; in others most relief is obtained by fomenting the head with water as hot as it can be borne.

The idea of employing hot fomentations in cases of this description was first communicated to me in 1833, by the late Mr. Swift, who became accidentally aware of their value whilst washing his face one day in very warm water, at a moment when labouring under severe headache. The sudden relief obtained by the application of hot water induced him to try it exclusively in the headache of influenza, and with the most satisfactory results. In the influenza which appeared in this country in 1833 and 1837, and again recurred in 1847, one of the most remarkable symptoms was intense headache. This was accompanied by great debility, and was not amenable to the ordinary modes of depletion. Now, in the first of these epidemics, Mr. Swift found that by applying water as hot as it could be borne to the forehead, temples, and back of the head, great and almost instantaneous relief was obtained, and that in this way he was able to keep a most unpleasant symptom in check, while he was taking measures to remove the disease. I have also heard from my friend Dr. Oppenheim, of Hamburg, that he had discovered that this was the best means of affording relief under the same circumstances. Mr. Swift's observations first led me to think of applying hot fomentations to the head in other diseases, and although I cannot give you any particular rules for determining the cases in which you should employ them, I can say that you will generally find warm vinegar and water the best and most efficacious application in the ordinary headache of fever.

I shall next offer you a few observations on the use of mercury in fever; and, first, are we to have recourse to mercury or not in typhus? I do not allude here to its use as an aperient; but, when called to treat a case of fever, are you to proceed at once to bring the patient's system under the influence of mercury? Are you, in addition to the other measures usually adopted in the treatment of fever, to go on with the administration of mercury until you affect the mouth, and bring on salivation? This was the practice in my earlier days, and great confidence was placed in it by the majority of practitioners. It has been also very extensively recommended by army and navy surgeons in the treatment of tropical fevers, but I must confess that I am not at all inclined to adopt this practice, and that I have seen abundant reasons why I should neither employ nor recommend it. In the first place, we have observed in our wards that patients with other diseases have frequently caught fever from exposure to infection, at a time when they were fully under the influence of mercury. In the next place, we have observed that persons who were thus attacked with fever while in a state of salivation did not escape better than others, and that in them the disease ran its full course, aggravated rather than diminished in its danger by the pre-existing mercurialization. These facts I have frequently seen verified in hospital and private practice.

You perceive, then, that mercurialization neither protects a man from the contagion of typhus, nor does it produce a favourable modification in its type or progress. Again, I have repeatedly witnessed the daily and continued exhibition of mercury in fever, and I cannot recollect a single case in which it appeared to check the disease, moderate its symptoms, or bring about a favourable crisis. I am aware that, in entering my protest against this practice, I dissent from a very considerable body of my brethren, who, from the beginning to the end of fever, never cease in their attempts to bring the patient's system under the influence of mercury. I am convinced that, in the cases in which recovery is stated to have followed this practice, the *post hoc* has been mistaken for the *propter hoc*. Besides, fever is one of those affections in which you will find it extremely difficult, and often impossible, to bring the system fully under the influence of mercury. There are certain states of the system which prevent altogether the full operation of mercury, and bad typhus is one of these states. Where fever has laid deep hold of the constitution, you cannot affect it with mercury. When a patient recovers who has been mercurialized during the course of fever, he does not recover because his system came under the influence of mercury, but he comes under the influence of mercury because he recovers from the fever. Add to this, that mercury is a remedy which requires a peculiar regimen, and that it is very apt to engross the practitioner's attention, and prevent him from the exhibition of remedies which are more directly indicated, and in reality more useful.

did not expect that the case would terminate fatally in such a short time, and I anticipated benefit from the remedy prescribed. He was ordered to take the tartar emetic solution in full doses; but, on visiting him next morning, we found that he had obstinately refused to take his medicine, and that his symptoms were greatly aggravated.

In delirium of this kind it is certainly very difficult to manage the patient, and we are frequently obliged to have recourse to force and stratagem to make him take his medicines. I regret extremely that this man's head was not leeched on his admission, as, from the state of his pulse, I think he would have borne it well. Eight leeches might have been applied to his temples, and repeated two or three times the same day, according to the state of his pulse and strength. I think I was wrong in contenting myself with ordering the tartar emetic solution and a blister to his head, and I should have anticipated, from the violence of his behaviour, that it would be very difficult to manage him.

In cases of this kind, where it is necessary to give tartar emetic (and this is one of the best remedies you can employ in cases of cerebral excitement in fever), you should be always prepared to obviate any omission arising from the obstinacy of the patient; and when he will not take his medicines voluntarily, you may secure its effects on the system in two different ways. In the first place, it may be secretly mixed with the patient's ordinary drink; and as such persons are generally thirsty, and seldom refuse drink altogether, an intelligent nurse will readily find means to make the patient take a sufficient quantity of it to secure its full effect on the cerebral circulation.

Another expedient which you may resort to in similar emergencies is to give the tartar emetic in the form of enema. I had recourse to this plan some time since, in a similar case of delirium, and with the best results. After leeching the head I gave the solution of tartarised antimony in enema; and this can be always done, whether the patient likes it or not, if you take care to prevent his struggles by confining him in a strait waistcoat. The best way of administering it is to dissolve two or three grains of tartar emetic in four or five ounces of mucilage of starch or isinglass, and inject it with the aid of a long flexible tube, so as to make the contents of the syringe pass high up into the bowel. In this way you can secure all the good effects of tartarized antimony, in overcoming the congestion of the brain, and procuring sleep.

In all cases of alarming congestion of the head in fever, I have been long in the habit of using tartar emetic in this way, if the stomach be deranged, and incapable of bearing it safely; and I can assure you that it is a most fortunate thing to have such a powerful resource in all cases of the kind. I have also not unfrequently given expectorant medicines in the same way, where from the state of the stomach, or the debility of the patient, the ordinary remedies could not be administered by the mouth with sufficient rapidity, or in sufficient quantity to produce the desired effect. In this manner I have often given the infusion of ipecacuanha—a remedy of very considerable value, and not sufficiently appreciated by most modern practitioners. I may also remind you that vomiting, and all the benefits derivable from it, may be likewise thus produced. Of course, the cases in which these expedients are required are comparatively rare, but the practical physician must be always prepared for such exigencies, and be provided with means of meeting them.

Another of our patients died also within the last few days in the fever ward. He laboured under a very bad form of maculated fever, and when admitted was evidently in a hopeless state. I shall not say anything about this case, except to use it as an occasion for making a few observations on a particular state of the cerebro-spinal system, which we not unfrequently observe in cases of maculated typhus, and occasionally in other varieties of fever. Now you observed that this man had not the slightest tendency to sleep; that he lay with his eyes constantly open, raved incessantly, had subsultus tendinum, floccitatio, and cold extremities, and often attempted to get out of bed. Yet we could not find in him anything like decided evidence of cerebral inflammation. The tunica adnata was of a clear pearl white, the face pale, and the scalp and integuments of the face cool. You perceive, then, that sleeplessness, delirium, and subsultus tendinum may depend on a state of the nervous system having no connexion with congestion of the brain, or determination of blood to the head. This occurrence has struck me very forcibly in many cases of fever. But I have been most particularly struck with the occurrence of subsultus tendinum in such instances. In the present case we had a patient with sleeplessness and subsultus. But this concurrence of symptoms does not always exist.

You recollect the case of the boy in the small fever ward, who laboured under excessive subsultus, and to whom we gave the oil of turpentine in drachm doses with so much benefit. Yet this boy, as you all remember, slept remarkably well. I have frequently pointed out to the class, patients labouring under subsultus tendinum, who slept well, and in whom the tunica adnata was of a pearl white colour, without the slightest suffusion. We have subsultus, therefore, occurring in two very opposite states of the nervous system; we have it accompanied by loss of sleep, and we have it existing in that condition of the system where the patient slumbers long and heavily, and cannot be easily roused. Hence I am inclined to think that the cause of subsultus resides not so much in the nervous centres as in their extremities.

I would even go so far as to advance the proposition, that if it were possible for the fever to go on, and life to continue after the removal of the brain and spinal cord, I am quite sure that the subsultus would continue. I am almost confident that subsultus tendinum is the result of some derangement of the nervous extremities. I shall show hereafter, when lecturing on the subject of paralysis, that the nervous periphery may become diseased primarily, and without any antecedent affection of the brain or spinal cord. I think it extremely probable that in fever the nervous centres are subject to certain derangements producing coma, sleeplessness, and delirium, but that there are other nervous symptoms which are to be referred rather to a derangement of the nervous extremities, and among the latter I would particularly include subsultus tendinum, a symptom which we find co-existing with such opposite conditions of the nervous centres.

But to return to the case to which I first alluded. Never blister in the early stage of fever, until you have applied leeches in sufficient quantity. In this case, it is true, we could not well ascertain what the period of the fever was; for the man was brought in, in a state of delirium, and there was nothing known respecting his previous history. You are all aware that a great deal must depend on our knowledge of the period of the fever, and the medicines which have been employed. Had we been acquainted with these circumstances, it is probable we would not have fallen into the error we committed. What I wish to impress on you is, that in all cases of maculated typhus, you should be careful in examining the head, and ascertaining whether there are any evidences of cerebral congestion present. If there be headache, strong pulsation of the carotids, suffusion of the eyes, and heat of the face and scalp, along with the other signs of functional lesion of the brain present, you should have recourse to leeching; beginning cautiously, and continuing their application as long as the patient will bear it with safety. When you have the symptoms already mentioned, and the patient is in the early stage of fever, you may commence by applying one or two leeches to the nostrils, or six or eight to the temples or behind the ears, repeating them two or three times a day, according to the exigency of the case. The best way of using leeches is to apply them in small numbers every six or eight hours, so as to keep up a constant drain from the head. After you have leeched sufficiently you may then have recourse to blisters. In making this change much will depend on the sagacity and skill of the practitioner; for it requires no ordinary tact to hit on the proper time when you should give up leeching and commence with blisters.

I shall make no apology for introducing here what I consider to be an important observation, with reference to the pathology and treatment of fever. We had a striking instance of the fact on which I am about to offer some comments, in the case of a little girl who died lately here, in a very remarkable manner. Every fever which commences with vomiting and diarrhœa, whether it be scarlatina, or measles, or typhus, is a fever of a threatening aspect; and in all such fevers the practitioner should be constantly on the watch, and pay the most unremitting attention to the state of the brain. There is much difference between the vomiting and diarrhœa of gastro-enteritis and this *cerebral diarrhœa and vomiting*. The latter sets in generally at a very early period of the disease, perhaps on the first or second day, and is seldom accompanied by the red and furred tongue, the bitter taste of the mouth, the burning thirst, and the epigastric tenderness which belong to gastro-enteric inflammation.

There is also another source of diagnosis, but of a less valuable kind; and this is founded on the results of treatment.

Gastro-enteric vomiting and diarrhœa are relieved by leeching the belly; but I need not tell you that this mode of treatment can have no effect on the vomiting and purging produced by cerebral disease. There is also another means of distinguishing: the vomiting and diarrhœa which result from gastro-enteric inflammation are never accompanied by such copious discharges of bile as when they depend on disease of the brain. In diarrhœa from derangement of the brain the quantity of bile passed is very remarkable; and it is equally curious, that when vomiting follows derangement of the cerebral circulation, in ordinary cases, and without fever, bile is thrown up in very large quantities. This is frequently observed in persons who become sick from swinging or sailing. In such instances a larger quantity of bile is vomited than could occur from mere gastric irritation. Now, in the commencement of cerebral disease, where congestion or inflammation is present, one of the first symptoms is copious vomiting and purging of a bilious character. This is very often the case in scarlatina, and there are few cases in which there is more danger to be apprehended. We had these symptoms, under very unfavourable circumstances, in the little girl to whom I have just alluded. From the imperfect history of the case which we were able to obtain, it appeared that she had been ill of fever for fourteen days before her admission, and had in addition a severe attack of bronchitis and pneumonia. She then got inflammation of the stomach, and finally congestion of the brain, as indicated by the cerebral vomiting and purging. We employed every means in our power to check these symptoms, but without success; she went on from bad to worse, and she ultimately sank under a combination of affections, which you will frequently observe in many forms of disease as well as fever; and it is to this point in particular that I wish to direct your attention.

You will frequently observe that at a certain period of fever, whether it be inflammatory, nervous, bilious, or typhoid, and very often in other forms of disease, whether depending on a general affection of the system, or connected with inflammation of important organs, when the patient has been going on pretty well for some time,—you will find that about the period when you would naturally expect the fever to go off, and convalescence begin, a new form of fever makes its appearance, and carries off the patient in spite of all your exertions. To this form of secondary fever I would give the name of scrofulous, because it resembles in its chief features the intractable form of fever which is frequently observed in persons of an originally scrofulous habit, or who have become so from the abuse of mercury, or other debilitating causes.

This is a subject which is not well understood, and I am not acquainted with any author who has devoted to it that share of attention to which, from its great importance, it has such decided claims. Its chief characters are that the patient, during its existence, exhibits a strong tendency to inflammatory affections which bear a close analogy to the scrofulous, both in their intractable character, in the facility with which they pass from one organ to another, and in their frequently unfavourable termination. A patient of this description, while labouring under fever, will frequently exhibit a very remarkable succession of inflammatory affections. If, during the course of his fever, he gets an attack of gastro-enteritis, you will have great difficulty in managing it; and no sooner is this overcome than he is seized with bronchitis or pneumonia; and when, by great care and the most skilful treatment, you have overcome this also, he gets scrofulous inflammation of the brain, and dies.

Now you will frequently meet with patients who, during the course of typhus, will be attacked with this bad form of fever, and get what may be termed scrofulous inflammation of the brain, which carries them off in five or six days, in spite of all your care. You are aware that persons who are much in the habit of observing diseases of the brain, can generally distinguish between *scrofulous* inflammation of the brain and its membranes, and that inflammation which occurs in persons of healthy habit. In cases of the latter description, the treatment, if commenced at the first appearance of the disease, is simple and successful. Appropriate bleeding and leeching, with the use of calomel and James's powder, are almost always sufficient to accomplish a cure. When once you have succeeded in touching the gums with mercury, the patient's safety is tolerably certain, and recovery is in general rapid. But in the scrofulous affections of the brain, although you may have fully mercurialized your patient, you will too often discover that you have merely retarded the progress of the complaint for a brief period; it grows bad again, and carries him off in spite of all your efforts.

In the scrofulous hydrocephalus, a much greater time elapses from the appearance of coma and strabismus until death takes place, than in the ordinary forms of meningitis. This fact was well illustrated in the case of the girl to which I have just now referred: she continued to live on for a long time after the appearance of symptoms which you would think ought to terminate fatally in a few hours after they had been developed. There is also a great deal of irregularity in the way the symptoms come on in cases of scrofulous inflammation of the brain. Sometimes blindness is one of the first symptoms. I recollect having been called, with Dr. Beatty, to see a very fine boy living in Merrion Square, and was very much struck, on entering the drawing-room, to find him walking about, and in apparent good health, but quite blind. Here amaurosis was the first symptom. This was subsequently succeeded by others, and he died in a convulsive fit about a fortnight afterwards.

We have many excellent observations on the chronic scrofulous fever, but I think that there is no author who has described this acute form with the precision and care which it deserves. It is, however, a very frequent form of fever, and you will see many examples of it among the chronic patients in the medical and surgical wards. You will frequently observe persons who are labouring under acute disease, from accidents or other causes, become feverish and ill again at a time when you expected a remission of their symptoms, or even recovery; and, without any assignable cause, they will get scrofulous inflammation of some other part or organ, and quickly fall into a state of hopeless and incurable disease.

LECTURE XIV.

HEAD SYMPTOMS IN FEVER.

I SPOKE at my last lecture of a man named Cassels, who died in the fever ward with symptoms of cerebral excitement, and stated that I regretted having omitted to leech his head, and prescribe tartar emetic in the form of enema. Since that time we have had an opportunity of examining his body, and the results of the dissection are well worthy your attentive consideration. He was a young man of robust habit and apparently good constitution, and laboured under the ordinary form of maculated typhus. Shortly after his admission he was attacked with delirium, which was soon afterwards followed by coma and death.

Now, suppose you were called to see a patient not labouring under typhus, but exhibiting a similar train of symptoms—that is to say, violent delirium, accompanied by flushing of the face, suffusion of the eyes, headache, and a tendency to get out of bed—in fact, a state of furious excitement requiring the restraint of the strait waistcoat—what idea would you be likely to form of the condition of the brain? If a patient of this kind had no typhoid symptoms, you would certainly say that he was labouring under meningitis or cerebritis; and, if the case proved fatal, you would naturally expect to find lesions of the brain fully sufficient to account for all his symptoms. And you would in all probability find extensive thickening of the membranes of the brain, with sub-arachnoid effusion, or you would discover softening, increased vascularity, and suppuration of the encephalic mass.

But here a man in fever exhibits all the symptoms of cerebral inflammation; the cerebral affection runs on to a fatal termination with great rapidity; he dies comatose. And what do we find on dissection? Doubtful signs of congestion, and no distinct evidence of inflammation, a slight opacity of the arachnoid at the base of the brain, and about a teaspoonful of clear subarachnoid effusion. Now this is a point to which I would earnestly call the attention of every inquiring student. A patient, during the course of typhus, is seized with symptoms which are generally regarded as characteristic of congestion and inflammation of the brain; he dies, to all appearance, in consequence of the intensity and violence of these symptoms, and, on dissection, little or no trace of cerebral disease is found. In the case under consideration, the symptoms present were strongly indicative of congestion, if not of inflammation; and had the man been free from typhoid symptoms, you would expect to find decided traces of inflammatory mischief. This seems to prove that, in the production of cerebral symptoms in typhus, some cause not to be recognised by the production of cerebral lesions, or in other words something besides mere congestion or inflammation, exists. I have now examined a great number of cases of this description, and the examination has brought home to me a strong conviction, that the delirium of fever depends upon something more than mere inflammation or congestion.

There is another fact, the study of which is well worthy of attention, as it appears to support very strongly the views I have put forward; and that is the occurrence of analogous symptoms under opposite conditions of the cerebral circulation. Take, for example, the phenomena of vertigo and headache. Now these symptoms are found in states of the brain which are directly opposite. In incipient congestion of the brain, in that turgescence of the cerebral vessels which precedes apoplectic seizures, one of the most frequent symptoms is vertigo, and the same thing may be affirmed with respect to headache. But we observe the very same symptoms under circumstances totally dissimilar. Frequently, while bleeding a patient for some affection of the lungs or bowels, or for some accident, we find that after a certain quantity of blood has been lost, the patient becomes pale; and while the pallor is coming on, he often gets quite giddy, and sometimes complains of headache. Gentlemen who are attending lying-in hospitals are well acquainted with the headache, giddiness, and tinnitus aurium, so constantly complained of by females who have suffered from excessive uterine hemorrhage.

Hence you perceive facts are not wanting to show that opposite states of the cerebral circulation, a superabundance or deficiency of pressure on the brain, may give rise to similar phenomena. You saw an illustration of this in the case of one of our patients in the fever ward this morning. He was quite free from headache as long as he remained in the horizontal posture, but the moment he sat up in bed he complained of headache. Yet this was a man who had not the slightest symptom of determination to the head, and who had been sufficiently depleted during his illness. You will also recollect the fact, that persons who have had a long illness, and remained for many days in the horizontal posture, generally get weakness, giddiness, and sometimes headache, when they first attempt to sit up during convalescence.

This is a point which should be always borne in mind. You are consulted by one person who complains of giddiness, tinnitus aurium, and frequently recurring headache. You examine the patient carefully, and you find all the symptoms of unequivocal determination to the head. You are applied to by another person labouring under the same symptoms; but how different is the state of the brain found to be on a careful examination. One patient is robust, of florid complexion, and with a hard bounding pulse; the other is a weak chlorotic female, who has been ailing for months, and whose pulse is so weak that a slight degree of pressure obliterates the canal of the artery. Yet the tinnitus aurium, giddiness and headache complained of by the latter are just as bad and as troublesome as in the case of the former.

From a consideration of these points you will perceive that, for the production of cerebral symptoms in typhus, there must be something more than mere congestion or inflammation of the brain; but you are not to infer from this that there is no necessity for taking any steps to obviate or remove congestion of the head in fever. On the contrary, I am of opinion that in typhus one of the principal sources of danger is connected with the head, and that the cerebral symptoms should be always watched with the most unremitting and anxious attention.

It is this which constitutes the great difference between the mortality in private and hospital practice. In private practice the physician is called at an early period of the disease, and has an opportunity of checking the cerebral symptoms before they rise to a dangerous height; but hospital patients, in general, are admitted at an advanced stage of fever, and in many instances came on, but probably our treatment prevented them from being fatal; for when they appeared, the application of tartar emetic ointment induced a purulent discharge from the whole surface of the twice blistered scalp, in the course of a few hours, and three grains of tartar emetic given in divided doses that day procured a complete cessation of the symptoms, after—mark—after the pupils had been dilated, and one fit of slight paralysis of the mouth and tongue had taken place.

The result of all my experience in fever is, that the majority of fatal cases are rendered so, in this country at least, by severe cerebral symptoms supervening sooner or later in the disease. Delirium, sleeplessness, stupor, convulsions, extreme subsultus, jactitation, sluggish and dilated, or else extremely contracted, pupils—these are the symptoms we have to fear after the fever has lasted some time; and, let me repeat it, the chief art of the physician consists not so much in remedying these symptoms as in anticipating them. When he judiciously attempts this, he may not, indeed, always succeed in preventing their supervention, but he will, in many cases, be successful in diminishing their violence, and preventing their usually disastrous effects.

Permit me next to direct your attention to the case of the patient Murphy, who died last week. This case excited a good deal of our attention at the time, and I wish to make some further observations on it while it remains fresh in your minds.

It was one of those mixed cases of typhus in which, as the fever advances, we observe the usual phenomena of determination to the head, accompanied by a train of symptoms which bear a close analogy to those of delirium tremens. Among the pauper population which we have to treat, you will frequently meet with cases of this description. We witnessed many examples of it here, but not so many as are to be seen in other hospitals. It is a melancholy but well-known fact, that a great proportion of the diseases which come under our notice, in the acute as well as in the chronic form, are more or less complicated with intemperance.* This you should never forget. In persons of the lower class, who are addicted to the daily use of spirituous liquors, you will find disease assuming a thousand unfavourable shapes and complications. You will find their fevers intermixed with various symptoms of an anomalous or dangerous character, and their chronic affections embarrassed by organic and visceral disease. You will be repeatedly struck with the strange and protean character which disease assumes under the influence of an habitual intemperance; and you will often, in the course of your practice, have to endure the annoyance and disappointment of seeing your patient carried off by some new and unexpected malady, after you have succeeded, by infinite toil, ingenuity, and prudence, in removing every trace of his primary affection.

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* Since this lecture was delivered, a great change for the better has been effected by the efforts of the Rev. Mr. Matthew; the poorer and working classes of Ireland are now for the most part distinguished for temperance.

The case of Murphy was one of those which have been neglected in the beginning, where the vantage ground has been lost, and the chances of success are diminished almost to nothing. You have observed that all the fatal cases of fever which we have had in hospital were cases admitted at an advanced period of the disease, and in which the head had been neglected. You have also observed how exceedingly difficult it must be to treat cases of this description. The patient is admitted at an advanced stage of fever, and at a period when he can give no account of his present or past symptoms, or the mode of treatment to which he has been submitted. He comes in with delirium, or coma, and subsultus tendinum; his symptoms are certainly cerebral, and he exhibits, perhaps, a blistered scalp; but we can have no means of ascertaining whether he has had headache, heat of scalp, throbbing of the carotid and temporal arteries, or vertigo,—we cannot, in fact, decide with precision as to the exact state of the brain, and our practice must be embarrassed by more or less doubt and obscurity. I have already impressed upon your attention the urgent necessity of watching the head in fever, and I think I cannot too often reiterate the advice which I have given you, to endeavour to check cerebral symptoms before they amount to any degree of absolute danger. The fate of those who have died here will convince you that when cerebral disease has once arrived at its acme, the most energetic measures will often fail in arresting it. It is a matter of vital importance, then, to prevent this lamentable state of things, and, as I have already remarked in this lecture, without waiting until the symptoms of cerebral disease manifest themselves, to anticipate its very origin, and thus be enabled to control with certainty, symptoms which assume such a fearful aspect in cases where cerebral disease has been allowed to go on unregarded. This is the practical lesson which I wish you to draw from the four fatal cases which have occurred in this hospital within the last month.

There are some points in the case of Murphy to which I wish to recall your attention, as I am anxious that you should make them the subject of reflection. For some days before his death he had been delirious and unmanageable, with total loss of sleep, and a contracted state of the pupil. The antiphlogistic and derivative treatment had been employed without effect; and seeing that his symptoms were advancing, and his sleeplessness undiminished, I ventured to give him an injection, consisting of two grains of tartar emetic with ten drops of laudanum. I am cautious in the administration of opium in the advanced stage of fever, where there is evidence of determination to the head; and it was on this account that I ordered it to be combined with tartar emetic, giving also directions that the effect of each dose should be carefully watched. He got three enemata during the course of the night—that is, thirty drops of laudanum altogether. He dozed after the last injection, and appeared more tranquil; but at our morning visit we found him in a state of coma, with rapid sinking of the powers of life, and death took place in the course of a few hours afterwards. I must confess the issue of the case gave me some degree of uneasiness at the time, as I thought it might have been precipitated by the administration of the opium. I could not say but that even this small quantity of opium might have greatly aggravated the cerebral symptoms, and accelerated the fatal event.

Dissection, however, revealed the true cause of death. On opening the brain, we found extensive arachnoid inflammation, some effusion on the surface of the brain, and an intensely congested state of its vessels. The patient, altogether dissipated in his habits, and greatly reduced by fever, had been a young man of rather robust constitution previous to his illness; he had been neglected in the beginning of his fever, which, from the phenomena observed after death, must have been characterised by early and decided determination to the brain, producing delirium, watchfulness, coma, and a contracted state of the pupil, which all our antiphlogistic measures were inadequate to remove or control. We did everything in our power: we leeched, blistered, we meet with the occurrence of analogous symptoms in cases which exhibit a very different state of the brain after death. We are encountered with the same puzzling contrarieties in many cases of scarlatina. Cases come under our notice in which the patients appear to die entirely from the violence of the cerebral symptoms, and yet, on examination, we find very dissimilar states of the brain. In some, there is palpable and fatal lesion; in others, there are some dubious marks of congestion, quite insufficient to account for the symptoms; or the brain is found to be perfectly sound and normal.

It would appear that in scarlatina and fever, the poison of the disease exercises a deleterious influence on the brain, independently of inflammation, but capable of producing an analogous train of symptoms. Hence it is in many instances extremely difficult to distinguish the cerebral symptoms produced by the poisonous influence of fever on the brain, from those which depend on true inflammation. The one gives rise to delirium and fatal coma as well as the other; and in the advanced stage of fever, when the manifestations of nervous energy are feeble and imperfect, and when the circulating and respiratory organs act with diminished power, the distinction between mere irritation and actual inflammation becomes a matter of great difficulty.

In alluding to the occurrence of analogous symptoms under opposite conditions of the brain, I noticed that headache, tinnitus aurium, and giddiness, have been observed in cases where there was distinct evidence of determination to the head, as well as where there was every reason to believe that the supply of blood to the brain was greatly diminished. You will find a very curious illustration of this fact in the first volume of Guy's Hospital Reports, which contains a very interesting paper from Sir Astley Cooper, on the effects produced by tying the carotid and vertebral arteries. Among other results, it appears that when the supply of arterial blood destined for the brain is diminished, the animal experimented on becomes stupid, is to a certain extent incapable of voluntary motion, and exhibits a very remarkable dilatation of the pupils. This is an extremely curious fact. You are all aware that dilatation of the pupils has been long regarded as one of the most characteristic signs of extravasation and increased pressure on the brain; and yet it appears the very same condition of the pupil is observed when you cut off the supply of arterial blood to the brain. We are, I fear, as yet very much in the dark as to the derangement of function which occurs in the brain under opposite states of its vessels; and I think we have equally imperfect and confused notions of the changes which take place in that organ as the result of fever.

Dilatation of the pupils is usually regarded as a sign of increased pressure on the brain; and when hydrocephalic symptoms are present, it is generally looked upon as pathognomonic of effusion. Yet from the experiment just alluded to, we find that dilatation of the pupil is also the result of a state of things in which we cannot suppose the existence of anything like increased pressure on the brain. When I speak of increased or diminished pressure on the brain, I am not prepared to maintain that such is actually the case, or that when a man becomes giddy and faints after bleeding, the actual quantity of blood circulating in the brain is diminished, and consequently the amount of pressure; but when a man gets headache, vertigo, or syncope, from the loss of blood, it must depend upon causes different from those which are connected with congestion of the brain, or extravasation on its surface, or into its substance. What I wish to impress upon your minds is, that dilatation of the pupils may be connected with very opposite states of the cerebral circulation; and that in fever it cannot of itself be regarded as a sign of paramount value in determining the existence of congestion or inflammation of the brain.

It may not be amiss to mention briefly on the present occasion, the details of a very remarkable communication, by Surgeon Russell of the 73rd regiment, formerly a pupil in this hospital. This communication was read by Dr. Wilson, at one of the *soirées* of the College of Physicians in London, and afterwards published in the *Medical Gazette*. Mr. Russell observes:—

"I was led, by the following circumstance, to reflect on the nature of *coup de soleil;* which, as well as I can recollect, is treated of by all authors, and is generally considered to be nothing more or less than true apoplexy, produced by the direct influence of the sun's rays; that its pathology is the same, and its mode of treatment similar—that is, that all the efforts of the medical attendants should be directed to the head, as the chief, nay, almost the only, seat of the disease; and here it strikes me a fallacy exists, leading to erroneous principles of practice. In May, 1834, while I was in medical charge of the 68th regiment (a fine corps, composed of men in robust health), then recently arrived at Madras, the funeral of a general officer took place; to which, unfortunately, the men were marched out at an early hour in the afternoon, buttoned up in red coats and military stocks,—at a season, too, when the hot land winds had just set in, rendering the atmosphere dry and suffocating even under shelter of a roof, and when the sun's rays were excessively powerful. The consequence was, that after proceeding two or three miles, several men fell down senseless. As many as eight or nine were brought into hospital that evening, and many more on the following day; three died—one on the spot, and two within a few hours. The symptoms observed (and they were alike in these three cases) were, first, excessive thirst, and a sense of faintness; then difficulty of breathing, stertor, coma, lividity of the face, and in one, whom I examined, contraction of the pupil. The remainder of the cases, in which the attack was slighter, and the powers of reaction perhaps greater, or at all events sufficiently great, rallied; and the attack in them ran on into either an ephemeral or more continued form of fever. The symptoms of these three cases did not more closely resemble each other than did the post-mortem appearances. The brain was, in all, healthy; no congestion or accumulation of blood was observable; a very small quantity of serum was effused under the base of one, *but in all three the lungs were congested even to blackness through their entire extent;* and so densely loaded were they, that complete obstruction must have taken place. There was also an accumulation of blood in the right side of the heart, and the great vessels approaching it."

Since our last meeting, some cases of fever have occurred in our wards, which have presented too many points of interest to be passed over without any observation. A very curious case occurred here, in a man named Toole, who was admitted on the 4th of January. This patient is a robust labouring man, about thirty years of age, and had been ill with fever for ten or eleven days before admission. Of his previous history we could learn nothing; but when he came under our care he appeared very ill, and exhibited great depression of the vital energies, so that we found it necessary to encourage reaction by the application of heat to the surface of the body, frictions, warm fomentations, and the internal administration of wine and carbonate of ammonia. On the following night reaction became established; next day he became irritable and restless, and towards night was seized with delirium. The nurse omitted to report his state to our apothecary Mr. Parr, or the resident pupil; he was thus left without any treatment until next morning. Now, this is a matter of much regret to me, and I think I cannot do a more essential service to those who are about to enter on the practice of their profession, than to impress, as strongly as I can, the indispensable necessity of watching fever patients with the most anxious and unremitting diligence. In a case of bad fever a single visit in the day will never suffice; two, and even three visits will be required; and when the patient is in a doubtful or dangerous condition, it will be often necessary to have a properly educated medical person in constant attendance, prepared to meet every emergency, and counteract or modify every unfavourable change. Fever will often run on for several days without any change calculated to arrest our attention, or call for the adoption of any new measures, and yet, in the space of six hours an alteration may occur, of which the physician should have early and full information.

Well, this man remained without any treatment for several hours after delirium commenced. On the 6th we ordered his head to be shaved and leeched, and prescribed tartar emetic, in doses of a quarter of a grain, every second hour. Next day we found him as bad as ever. The tartar emetic had failed in diminishing the cerebral symptoms, and his delirium had rather increased. We found also, on inquiry, that he had no sleep for the last three nights. His pulse was weak and rapid, his eyes suffused, his restlessness and delirium such that he required a person to sit by him constantly, and prevent him from getting out of bed. Under these circumstances, we ordered five drops of black drop to be added to each dose of the tartar emetic mixture, of which he took an ounce every third hour, that is, about a quarter of a grain of tartar emetic. He took four doses of this during the night, and next morning we found that the delirium and sleeplessness continued still unabated, and that the man was sinking fast into a state of stupor and insensibility. He neither answered questions nor put out his tongue when desired; he had subsultus, and was muttering to himself with great volubility and rapidity of utterance. Indeed, his condition was such that I had no hope. Among other symptoms, I should mention that he had contraction of the pupils, a symptom of very unfavourable augury in fever. Having failed with tartar emetic alone, and afterwards with tartar emetic in combination with opium, I had now to seek for some other means of subduing cerebral irritation, and in this emergency had recourse to the use of turpentine—a remedy which I was inclined to adopt in preference to any other, as there was some fulness of the abdomen, and other symptoms indicating the existence of congestion of the intestinal mucous membrane. I therefore ordered two drachms of the oil of turpentine to be made up into a draught with a little oil and mucilage, and administered every second hour.

I was guided here by a knowledge of the fact, that turpentine exercises a very remarkable influence over many forms of nervous irritation. I can refer for illustration to many affections of the nervous system characterized by excitement, in which turpentine has been employed with the most signal benefit. Thus, we frequently find it a most valuable agent in the treatment of chorea, of epilepsy, and of the convulsive fits of children. We have frequently experienced benefit from its use in the treatment of spasmodic affections of the stomach and bowels; in hysteria, tympanitis, and the subsultus of fever we often derive from it the most rapid and effectual relief. You recollect a case of typhus which was lately under treatment in our wards, and of which one of the most prominent symptoms was general and continued subsultus; and you have all witnessed how much relief the patient obtained from small doses of oil of turpentine. Hence I was led to conclude that it might be employed with benefit in the latter stages of fever, where vascular excitement is greatly abated, and where the most prominent symptoms are irritation of the nervous centres, with more or less congestion of the gastro-intestinal mucous membrane. In this case, however, I must confess I used it as a last resource, and did not anticipate the very striking results which followed so unexpectedly. After the second or third dose the patient had two or three full motions of the bowels, and shortly afterwards fell into a sound and tranquil sleep, from which he awoke rational and refreshed.

LECTURE XV.

BED-SORES IN FEVER.—CONTAGION.—SYMPTOMS OF CONGESTION OR INFLAMMATION OF THE BRAIN IN FEVER.

A WOMAN has been admitted lately who had been labouring under fever for a considerable time before she came into the hospital. This poor creature seems to have been in very miserable circumstances during her illness; her bedding must have been totally neglected, and no attention paid to cleanliness, for on her admission, though nearly free from fever, she was covered with bed-sores to a frightful extent. Almost every point which had been subjected to pressure had ulcerated, and the ulcers went on undermining the skin, and committing terrible devastation in the areolar substance. Cases like this require great care and unremitting attention; it is on the exercise of an active and untiring humanity that the cure will mainly depend. In the first place, you are to recollect that the efforts of the constitution towards the re-establishment of health are impeded by the irritation of the sores; sleep is prevented, and the patient kept in a state of continued suffering, while a constant drain from the system is kept up by the ulcerative discharge, adding to the amount of existing debility. Hence a pseudo-febrile state arises, characterized by quick pulse, restlessness, and want of sleep, somewhat akin to that which is produced by scrofulous irritation. The appearance, however, of general excitement of the system should never prevent the physician from adopting every mode of strengthening the patient as much as possible. You will not succeed in removing this condition by an antiphlogistic regimen; the patient requires tonics and narcotics, with a nutritious but not stimulating diet. If you put him on a low regimen, and give anti-febrile medicines, you will do mischief, you will increase the existing debility, and add to the source of febrile excitement. Your practice should be to prescribe a nutritious diet, wine, and the sulphate of quina, and to treat the sores with stimulant applications. The local application which we find most beneficial in such cases, is one composed of two ounces of castor oil and one of balsam of Peru, which is to be applied on pledgets of lint and covered with a poultice of linseed meal two or three times a day. In addition to this, we direct the sores to be washed night and morning with a solution of chloride of soda, in the proportion of twenty or thirty drops of the saturated solution to an ounce of water. We also direct the patient to lie occasionally on her face, and enforce the strictest attention to cleanliness on the part of the nurse. Dr. Arnott's hydrostatic bed is an excellent adjuvant in the treatment of this disease.

Such, then, is an outline of our mode of treatment of bed-sores in fever. We order the patient nourishing, but not heating food; we give wine, regulating its quantity according to its effects on the system, and the liking of the patient; we prescribe small doses of the sulphate of quina, and administer an opiate at night to allay irritability, and procure sleep. The local treatment consists in the use of stimulant and detergent applications, poultices, attention to cleanliness, and change of position.

Let me, however, beg your attention for a few moments, while I dwell a little more at length on the subject of bed-sores, a very troublesome occurrence common to most cases of protracted illness, requiring the greatest attention and care on the part of the physician, and in the treatment of which much ignorance is too often displayed by young as well as old practitioners. If the duration of your patient's complaint renders him liable to such affections, how are you to act so as to obviate them? In the first place, you must pay particular attention to the state of his bed. One of the best modes I am acquainted with of preventing the formation of bed-sores is, to keep your patient perfectly clean, to shift him frequently, and to take particular care to prevent him from lying in the wet. A physician should never trust the arrangements connected with his patient's manner of lying to the discretion of nurses; he should always look to it himself. You are advised to make your patient change his posture to obviate the effects of pressure, and to use cushions of various kinds. All these rules are good. You are also told to wash the parts with camphorated spirits of wine when any discolouration appears. This, too, is useful. But, in spite of all this, after fever has continued for some time, and your patient has become debilitated, bed-sores will come on not only in consequence of the effects of pressure, but

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also from the tendency in the constitution to form those sores. You remember the case of a man who had a bed-sore under the skin of the sole of the foot, and another under that of the heel,—parts totally exempt from pressure.

When the first redness, indicating the approach of a bed-sore, has made its appearance, various other means are usually adopted. Some advise the application of pledgets of lint moistened with camphorated spirits, and they endeavour to keep these pledgets in contact with the part, by means of bandages or adhesive plaster. Others use dry lint, or hair-powder, and many are in the habit of immediately covering the affected portion of the skin with adhesive plaster alone. The latter application too often aggravates the mischief, by exciting a rash and itchiness in the surrounding integuments, which become an additional source of inconvenience, and often force the patient to scratch the irritated parts in such a manner as to disturb and remove all the dressings. You must recollect, too, gentlemen, that fever patients are always restless, and frequently delirious, and consequently they are constantly changing their position, and tossing about in the bed, so that it is quite impossible to make use of any contrivance capable of keeping these applications in their place. After they have been fixed on and adjusted with the greatest ease, if you return in a few hours you will find them, if not removed altogether from the part, so wrinkled and crumpled, as to form, by the inequality of their pressure, new sources of irritation. What, then, is to be done? What means do I recommend to enable us to avoid so serious an evil? A case of this kind cannot be too vigilantly watched, and it is only by the most anxious attention and care bestowed upon everything connected with the cleanliness, dryness, and comfort of your patient, that you can avert the formation of bed-sores in protracted and putrescent fever.

In private practice, I never treat a fever of this nature without having a second bed in the patient's apartment. After the eleventh or twelfth day, the patient is removed from one bed to another every twenty-four hours; and when the disease is still further advanced, particularly if the patient wets the bed, the removal may take place every twelve hours. The moment he is changed, all the foul sheets and blankets are removed from the apartment, and, if necessary, a fresh mattrass is provided. Many subsided under the use of this remedy; to me it was perfectly novel; but when we reflect upon its utility in erysipelas, we are only surprised that it was not before suggested in the treatment of *incipient* bed-sores.

With respect to the present epidemic fever,* we have now seen so many instances of its direct communication from one point to another, in our wards, that we are induced to believe it to be contagious. From the great number of applicants labouring under serious and threatening diseases, we are sometimes obliged to put into our fever wards patients affected with local inflammations, accompanied by symptomatic inflammatory fever; several of these, while recovering, have been attacked with symptoms of the present epidemic. A man was admitted last week into the fever ward with violent pneumonia; the right lung was extensively hepatized, and, in addition to this, the pleura was found to be engorged over a large portion of its surface. The case was one of extreme distress, and the state of the patient apparently hopeless; however, by appropriate depletion, assisted by mercury and blisters, convalescence became established, and the pulmonary systems were rapidly subsiding. His system was still under the influence of mercury, his fever had disappeared, his dyspnœa was relieved, his cough and all the other symptoms nearly gone, when he was suddenly attacked with fever, and that of the same character as prevailed among the patients in the same ward. This is, I believe, the sixth or seventh case in which patients labouring under some other form of disease have been seized with symptoms of the present epidemic, while lying in the same ward with fever patients. I have thought it necessary to make this observation, because you will find it asserted in medical works, and by physicians of considerable eminence, that in hospitals fever does not spread from one patient to another, and that where it does appear among many individuals in the same house, its spread is chiefly favoured by want of cleanliness and proper ventilation. This, however, we can state to be the fact, that fever will spread among patients in the same ward, independent of anything connected with filth or foul air, for we have seen it occur in our wards, which I can assert are kept as clean, and as well ventilated, as any in the kingdom.

There is one circumstance connected with this case worthy of remark, with reference to the supposed anti-febrile properties of mercury. It has been stated that mercury exercises a prophylactic influence over the system, and several persons who have cultivated medicine with success, but particularly some army surgeons of high authority, have asserted that the use of mercury not only cures fever, but also secures against it. I am afraid that in this and other cases mercury has more credit than it deserves. I have seen persons under the influence of mercury take cholera and die of it; and here we find a man whose mouth is still sore, in whom salivation had not ceased, getting an attack of fever at a time when he had just recovered from another disease. This shows that mercury is not to be looked upon as a prophylactic in cases of fever of a contagious nature. We cannot always cure or prevent fever with mercury; on the contrary, where fever of a particular kind is present, it prevents the constitution from yielding to its influence. Thus, in a case of hectic fever, brought on by suppuration of the liver, it has been found impossible to bring the system under the influence of mercury.

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* This observation applies to the epidemic of 1834.

There is a case in the female fever ward which requires a passing observation. A young woman, previously in the enjoyment of good health, was seized with symptoms of fever after exposure to cold; she got rigors, followed by headache, hot skin, thirst, nausea, and acceleration of pulse. It is unnecessary for me to detail the symptoms which attended her illness during the past week; I shall content myself with pointing out the symptoms which particularly attracted my attention to her case on Saturday morning. At that time her fever had increased; she complained of severe headache and restlessness; had foul tongue, thirst, and symptoms of gastro-intestinal irritation. Such matters demand no very particular consideration; what chiefly fixed my attention was the occurrence of slight and transient rigors during my examination: I observed her shuddering three or four times in the space of a few minutes. On questioning her respecting these brief rigors, she informed me that they had occurred with more or less frequency for the last three days. Now, whenever you meet with a symptom of this description in fever, be on your guard; watch the case with anxious, unremitting attention, and never omit making a careful examination. It is in this way that one of the worst complications of fever—treacherous and fatal disease of the brain—very often commences. On examining this girl, we found that she had not only headache, but also acute pain referred to the left ear, the external meatus of which was observed to be hot and tender to the touch. In addition to this, we were informed by the nurse that she had been seized with a sudden fit of vomiting shortly after we left the ward on the day before. Here was an array of threatening symptoms calculated to awaken attention in any, even the most heedless observer. A patient, after exposure to cold, is attacked with symptoms of fever; she has headache and restlessness; she then begins to complain of acute pain in the ear, darting inwardly towards the brain; and, finally, is seized with sudden vomiting. Under these circumstances, it is not difficult to form a diagnosis, and there can be little doubt but that the phenomena here presented were indicative of incipient inflammation of the membranes of the brain. It is not easy to say whether in such cases the inflammatory affection of the membranes precedes the external otitis, or whether the inflammation commences in the external ear and spreads inwards, though I am inclined to adopt the latter supposition, and the circumstance of the fever and earache arising from cold seems to give an additional degree of probability to this view of the question. Be this as it may, there could be no doubt but that this girl was, on Saturday, labouring under incipient inflammation of the membranes of the brain, as denoted by headache, rigors, acute pain in the ear, and vomiting.

Here let me observe, gentlemen, that in cases of this description, I look on the occurrence of external tenderness, not merely as an indication of an internal disease, but also as a favourable symptom. I have remarked that in all cases where this happens, the physician becomes more speedily and sensibly aware of the existence of internal disease, and the remedial means employed act with a more decidedly beneficial effect. I would prefer having to deal with an inflammatory affection of the brain or bowels, accompanied by external tenderness, and would feel much more certain as to the result, than if this symptom were but faintly marked, or totally absent. This observation is founded on experience.

In treating this case, you have seen that I have ordered relays of leeches to be applied in the vicinity of the affected ear until the earache has ceased. I have long followed this practice of applying a number of leeches in succession for the relief of local inflammation, and I can state with confidence that the result has been, in the majority of cases, highly satisfactory. Some prefer the application of a great many leeches at once; but my experience speaks strongly in favour of the practice of applying a small number, repeated at short intervals, until the violence of the local inflammation is subdued. Relays of six or eight leeches will suffice in the majority of cases of pectoral, cerebral, or abdominal inflammation. In some, however, when the attack is violent, fifteen or twenty must be applied at once; each succeeding relay may consist of a smaller number than that which preceded it. In this manner I have maintained a constant oozing of blood from the integuments over an inflamed organ, for twenty-four, or even thirty-six hours. In addition to this, I determined to bring her system rapidly under the influence of mercury, and, with this intent, administered calomel to the amount of a scruple in the twenty-four hours. These means have acted favourably, and she feels much better to-day.

Allow me to make one observation more which this case suggests. This young woman, you recollect, had, on her admission, some epigastric tenderness, which we removed by leeching, and she remained free from any symptoms of gastric irritation until last Saturday, when she got a sudden attack of vomiting. Now, *in all feverish complaints, where, during the course of the disease, the stomach becomes irritable without any obvious cause, and where vomiting occurs without any epigastric tenderness*, you may expect congestion, or incipient inflammation of the brain or its membranes. If called to a case of scarlatina, where there is severe vomiting, and perhaps diarrhœa, unaccompanied by thirst or epigastric tenderness, what should your practice be? Are you to direct your attention to the alimentary canal, and endeavour to arrest these symptoms? No. The vomiting here depends on active congestion of the head, and such cases are very apt to end in coma, convulsions, or death, from disease of the brain. You are all aware, that in cases of injuries of the head, followed by congestion of the brain, vomiting is one of the most prominent symptoms. The same thing occurs in febrile affections, attended with determination to the head. You are not to conclude that a fever is gastric, because it commences with nausea and vomiting; this is a serious, and very often a fatal mistake; yet I am sorry to say it has been committed by many practitioners, and I have been guilty of it myself. In such cases, you should not waste time in attempting to relieve gastric irritation by cold drinks, and leeches to the epigastrium, or to check diarrhœa by chalk mixture and opiates; you should direct your attention at once to the seat and origin of the mischief, and employ prompt and effectual means to relieve the cerebral congestion. Where the disease sets in with severe vomiting, unaccompanied by distinct evidences of gastric inflammation, whether it be common fever, or scarlatina, or measles, or small-pox, I commence the treatment by applying leeches to the head, convinced that in this way I shall be most likely to prevent an approaching dangerous congestion of the brain. I am anxious to impress this observation on your minds, because I am fully sensible of its importance, and feel certain that you will derive much advantage from bearing it in recollection during the course of your future practice.

There is another subject which I wish to bring before you to-day, namely, the seat of the swellings which, in the latter stages of fever, are usually attributed to inflammation of the parotid and sub-maxillary glands. Every writer on the subject of fever has noticed the occurrence, in the last stage of that disease, of tumors, which not unfrequently suppurate, and which all considered as the consequence of inflammation in the glandular system; the parotid and sub-maxillary glands being the parts most frequently engaged. Four such cases have lately presented themselves to our observation—two with a favourable, two with a fatal result. The latter afforded us an opportunity of examining the nature and seat of this affection, with a view of determining the correctness of the opinion generally entertained concerning these points.

According to the best authors, the parotid and sub-maxillary glands, towards the termination of fever, are liable to become painful, tender, and very considerably enlarged; and the tumour so formed is either a fatal symptom, or else, becoming the seat of a benign suppuration, proves salutary, or even critical. When of the former unfavourable character, they are said sometimes to attain to a considerable size in a very short space of time, and also to be liable to a disappearance equally rapid.

same degree of hardness; while that on the left side, which was only of two days standing at that period, was situated lower down, and was much less firm. These swellings subsided a good deal a few hours before his death. The areolar tissue, in the parts before enumerated, was not infiltrated merely with bloody serum, as in the other tumors, but this serum was everywhere mixed with pus, and the areolar tissue itself had become dense and friable, and was of a reddish, or rather a flesh colour. The parotid and sub-maxillary glands shared in this affection of the areolar tissue, and consequently contributed their proportion to the formation of the tumors; but they by no means constituted the whole of the swellings, or indeed anything like the greater portion of them.

A few days after Connor had been attacked, a similar swelling arose in a boy, named Byrne, who lay in the bed next to Connor; but it was confined to one side, and it occupied a position corresponding to the parotid, where it was most swollen; but in its less tumefied parts it extended both further downwards and backwards. This tumor suppurated, and formed an abscess, which was apparently much more superficial than the great mass of the swelling, and unconnected with it; for when it was opened, and its contents were discharged, the hardness and swelling in the region of the parotid seemed undiminished. The abscess, however, continuing to discharge matter, this swelling gradually declined, and finally disappeared.

At this very time a woman in the fever wards was attacked with a similar swelling, but which was evidently neither in the situation of the parotid or submaxillary gland: it was confined to the subcutaneous tissue immediately below the ear, and was prevented from suppurating by the application of leeches.

The facts just stated are, I think, gentlemen, conclusive in proving that the tumors hitherto supposed to arise from inflammation of the parotid or submaxillary gland, and which in fever sometimes forbode death, and are sometimes the precursors of returning health, are not owing to an affection confined in its action to these glands; but, on the contrary, the inflammation and its consequent tumefaction are seated in the areolar membrane of all the neighbouring parts; so that the bulk of the tumor is sometimes altogether, and generally for the greatest part, made up independently of disease of these glands. It would be rash to extend this conclusion to the mumps—cynanche parotidea—but I may be permitted to remark that I am far from being satisfied that the seat of the tumors, so called, has not been assumed without sufficient grounds. Indeed this disease so rarely, if ever, proves fatal while the swellings persist, that I do not know of any post-mortem examination of the tumors of mumps on record. Our only guide, therefore, is analogy; and when we recollect that our swellings agree with mumps, not only in general appearance and situation, but also in the sudden manner in which they arise, and, according to the testimony of authors, in the sudden manner in which they occasionally disappear; when we recollect, also, that like mumps, they show a decided tendency to be epidemic; we cannot avoid conceding that the points of resemblance are strong; the more so that in both diseases the sudden disappearance of the tumor is always dangerous. The sympathetic inflammation of the mammæ in females, and of the testes in males, which not very unfrequently follows retrocession of the tumors in cynanche parotidea, may be objected to this analogy, and may be considered as proving the glandular nature of the swelling in mumps. On the other hand, we know of no other glands which are liable to become, in consequence of inflammation, so enormously enlarged in the course of a few hours, as the parotids in mumps (if that disease really depends on an affection of these glands alone); and, indeed, it may be observed that acute inflammation seems, in all other glands, incapable of causing a degree of swelling at all comparable to that observed in mumps. The swelling, too, in other glands is better defined and more circumscribed, and scarcely liable to the sudden retrocession observed so frequently in cynanche parotidea. It is a singular fact that the salivary secretion is not notably altered in mumps, and yet were this disease dependent on inflammation of the parotids, a suppression, or at least some alteration, in the quantity or quality of that secretion might be expected. Such, gentlemen, are the ideas which have at the moment occurred to me concerning the pathology of these affections—ideas which I have ventured to bring forward merely with a view of exciting further inquiry on the subject.

LECTURE XVI.

TARTAR EMETIC AND OPIUM IN FEVER ATTENDED WITH CEREBRAL EXCITEMENT.

I HAVE several times alluded to the use of tartar emetic in the treatment of the cerebral excitement and determination to the head, which are so frequently witnessed in the advanced stage of typhus fever; I shall now proceed to mention in detail some of the beneficial effects derived from this plan of treatment, as illustrated by cases which have recently occurred in my own practice, or in that of other members of the profession.

Did I bring forward this plan of treatment as infallible, or if I boasted that it never failed, then, indeed, you might well doubt my judgment in recommending it to your notice, for infallible remedies never earn the sanction of experience; but such is not the fact. This treatment we ourselves have seen will not always succeed; nay, we must acknowledge that it has occasionally disappointed us, even where we seemed justified in calculating upon success. But, gentlemen, we must recollect that every useful remedy is subject to the same charge, and that in the long list of therapeutic agents there does not exist a single medicine which is fairly entitled to the appellation of a true and infallible specific.

We have failed in several cases with tartar emetic, either alone or combined with opium and other medicines, and patients labouring under typhus have fallen victims to cerebral disease, although we applied the remedy with all due diligence. Yet I think it but fair to observe, that most of the instances in which we failed were cases that had come under our notice at an advanced stage of fever, and where the cerebral symptoms had been wholly overlooked or improperly treated in the commencement of the disease. I may observe also, that cases of this description, in which the cerebral symptoms have been permitted, before admission into hospital, to form themselves fully, are exceedingly difficult to manage, and terminate fatally at a much earlier period than the ordinary cases of typhus observed in private practice.

Maculated typhus with determination to the head, when improperly treated, terminates not unfrequently about the tenth, eleventh, or twelfth day; sometimes it is protracted to the thirteenth or fourteenth, but most usually it ends fatally about the eleventh or twelfth. In neglected cases, the cerebral symptoms frequently assume a fearful violence on the seventh, eighth, or ninth day, and in such instances it must be expected that the best and most appropriate plan of treatment will fail in rescuing the patient from impending dissolution. If, however, we can find out a remedy which, in many cases apparently desperate, succeeds in rescuing the patient from the jaws of death, we must be satisfied. A case of this description has occurred since our last meeting. It has excited the attention of all who witnessed it, as well from the violence of the symptoms, and the apparently hopeless state of the patient, as from the rapidity with which the exhibition of the remedies employed was followed by a striking and decided alteration in the symptoms. Any one who saw him yesterday would scarcely recognise him as the same individual to-day.

This man, named Fogarty, was admitted about the seventh or eighth day of his fever, according to the account of his friends. Of course in such cases we cannot give implicit credence to those loose statements, for the lower class of persons in this country never calculate the time during which the patient remains out of bed struggling against the disease, a period which, in a people inured to suffering and privation, frequently lasts three, four, or even six days. Well, this man, aged five-and-twenty, and of rather robust constitution, was admitted on the 20th of December, being then about eight or nine days ill. Previously to admission he had taken purgative medicines, had his head shaved, and six leeches applied behind his ears or to his temples, I forget which. Now all these measures, although perhaps insufficient, were extremely proper, and must have produced more or less benefit. When we examined him on the 21st, we found him in a state of high excitement, as manifested by continued mental wandering, incessant talking and raving, and frequent attempts to get out of bed. He had illusions of the senses of sight and hearing, consisting of terrific ocular spectra, and alarming sounds, which threw him into a state of intense agitation;* his eye was red and watchful, and he never slept. Here, then, was a very threatening array of symptoms — perfect insomnia, ocular spectra, illusions of the sense of hearing, a fiery eye, and incessant mental wandering. To this was added great derangement of the whole nervous system. His body was agitated from head to foot by continual tremors, and he had violent and persistent subsultus; his respiration was interrupted, suspirous, and irregular, amounting at one time to 40 in the minute, afterwards not exceeding 25; the acts of inspiration and expiration were extremely unequal, and occasionally accompanied by blowing and whistling. In a former lecture I made some observations on this form of respiration, which I termed *cerebral*, from having first observed it in persons subject to apoplectic attacks, either before or during the paroxysms. It is frequently observed in bad cases of fever, and is a symptom of the greatest importance. He also lay constantly on his back; his pulse 120, soft, and very weak, so that the canal of the artery could be obliterated by very slight pressure; his pupils were somewhat dilated; tongue parched, and brown in the centre, red at the edges and tip; skin covered with maculæ; abdomen soft and full.

Those who have witnessed the case will acknowledge that the picture I have drawn is not too highly coloured, but, on the contrary, falls far short of the reality, and no doubt you all expected that if we did not succeed at once in arresting the progress of his symptoms, the case must have proved rapidly fatal. Observe the position in which we were placed. In the commencement of the fever, certain appropriate but inadequate remedies had been employed, and, under a treatment proper but insufficient, the disease had progressed. It was an example of one of the worst forms of fever, characterized by intense cerebral excitement, and accompanied by total want of sleep, persistent delirium, and excessive disturbance of the nervous functions. All these symptoms had come on gradually, and arrived at their acme at a period when the low and debilitated state of the patient precluded the use of depletive measures to such an extent as to exert any efficient control over the most dangerous symptoms. The application of a few leeches would be extremely hazardous, and blistering would have been wholly useless and nugatory, for before the blister could rise the man would be dead.

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* In my last lecture I mentioned that analogous symptoms result from increased or diminished sanguineous pressure on the brain; the ocular spectra in Fogarty's case evidently depended on determination of blood to the head, but in the case of a lady, the wife of an eminent physician, a continued and varied succession of spectral illusions formed one of the chief symptoms, produced by exhausting hemorrhage after delivery.

For these reasons, we concluded that the only remedy we could have recourse to with any prospect of success was tartar emetic. We therefore ordered a draught composed of two drachms of mint water, two of common water, and a quarter of a grain of tartar emetic, to be given every hour, until it produced some decided effect on the constitution. You will recollect here that the scale was vibrating between life and death, that it was necessary that our plan of operation should be at once prompt and prudent, decisive and cautious. One of the pupils promised to stay by him the whole day, and watch the effects of the remedy, and I determined to visit and examine him personally in the afternoon.

In the course of four hours he took four doses of the tartar emetic; the first and second, in fact almost every dose vomited him, but not immediately. He retained each dose for a considerable time, and then threw it up. After the fourth dose, it began to act on his bowels, and then the medicine was suspended for some time, and a small quantity of porter administered. When I saw him at eight o'clock in the evening he had been freely purged, and had discharged a considerable quantity of bilious yellow fluid from his bowels. He had also enjoyed about an hour's sleep; his respiration was now more uniform and natural; his raving greatly diminished; the subsultus and tremors were nearly gone, and the man appeared quite tranquil. I then ordered him a wine-glassful of porter, with two drops of black drop, to be repeated every second hour for three or four turns successively. I saw that the cerebral symptoms were evidently diminished, and that there was a tendency to returning tranquillity and repose, and I wished to follow up and assist the operations of nature. To-day this man is in a most favourable state. His skin is covered with a most profuse warm perspiration; he has slept well; belly soft and natural, respiration slow and regular, and pulse diminished in frequency. He is calm, rational, and she made no answer, but kept her eyes steadily directed towards the foot of the bed. Her aspect was altogether frightful, and Dr. Beauchamp observed that her state appeared to be a combination of delirium with hysteria.

"The question now was, what were we to do? We dared not apply leeches, blisters would be doubtful, and the probability was that the patient would sink before they vesicated. There was no indication for cold to the head, for the scalp was cool. Could we rely with safety on nervous medicines? Their very stimulus might hasten her to the tomb. The indication was to relieve the brain; and the question was, what medicine or combination of medicines would effect this with safety? Under these circumstances, we happily thought of the treatment employed by you in somewhat similar cases. We immediately ordered a mixture containing three grains of tartar emetic, half a drachm of laudanum, and six ounces of water; of this a tablespoonful was administered every half hour, its effects being watched. We saw her again at one o'clock on the same day, and had the pleasure of finding her much improved. She had taken three doses, and vomited twice. The expression of her countenance was much changed, it had lost its ferocity and wildness; her tongue was now moist, perspiration was beginning to appear over her body, the pulse was soft and about 100, and the intelligence, which had been absent for a considerable period, now reappeared. She was able to answer our questions, and expressed herself relieved. We ordered the medicine to be continued, giving a tablespoonful every hour. After taking two doses, she became perfectly quiet, fell into a profound and tranquil sleep, perspired copiously, and at our visit next morning at ten o'clock we found her, to our astonishment, almost well. She looked cheerful and refreshed, and spoke of the wonderful relief she obtained; her pulse was soft, and about 80; her skin natural, and her tongue moist and clean. Dr. Beauchamp did not think it necessary to continue his visits, and all that remained for me was to conduct her by proper regimen from convalescence to perfect health. She is now quite well.

"It is a source of gratification to me to have had the able assistance of Dr. Beauchamp on this occasion, and his presence during the eventful period adds much value to the case. Dr. Beauchamp remarked, at the time when hope had fled, that he knew of no routine of practice which afforded a probability of being of service, so that we may fairly conclude, that but for your happy combination the patient must have died."

This is a very strong case; indeed there could scarcely be a more striking illustration of the value of tartar emetic and opium in the treatment of the cerebral symptoms of fever. The case, too, was one of great danger; the patient was of a nervous weakly habit, and during the acme of the disease she had an attack of convulsions. This is a very important and most formidable symptom in fever, particularly when superadded to others indicating a deranged state of the sensorium. We had a patient here, some time back, who had two convulsive paroxysms during the course of his fever, and you recollect that I told you that it was a symptom of unusual danger. Some time ago a gentleman, in discussing my cases, said that convulsions in fever were not so dangerous, but I had the satisfaction of quoting for him the authority of Hippocrates, to show that persons who had been attacked in this way very seldom recovered.

I shall next detail a very remarkable case, which was communicated to me by Mr. Swift:—

"J. Kinsela, a labourer, aged 23, of powerful make, and robust constitution, was attacked with fever about the 14th or 15th of January. He complained during the ensuing week of intense headache, thirst, and debility, but had no medical treatment. On Saturday, the 21st, he was extremely ill and restless, and on Sunday morning, while his clergyman and several of his friends were with him, he got out of bed in a state of furious delirium, seized a knife, and having cleared the room, rushed out into the street in his shirt, where he was secured by a policeman and some of his neighbours, and brought back to bed, having previously wounded several of his captors in the struggle. He then fell into a state of coma, and when I saw him on the following Thursday, the 26th, he exhibited the following symptoms:—Decubitus on the back; eyes nearly closed; lips red, dry, and chapped; forearms bent and agitated by apparently unconscious movements; convulsive twitches of the eyebrows and angles of the mouth; breathing irregular, heavy, and somewhat stertorous (of that description which you have aptly termed *cerebral*); pulse oppressed, unequal, weak, and about

110; great heat of scalp and face; temperature of the body normal; feet very cold. He had no pulmonary symptoms; his belly was soft and apparently natural, but he gave indications of uneasiness when firm pressure was made over the situation of the stomach and small intestine. He was raised up in bed, shaken roughly, and spoken to repeatedly, but gave no answer; nor would he put out his tongue, or open his eyes when requested. His tongue, as far as I could see it, appeared red, dry, crusted, and fissured; and on raising his eyelids, I found the eyes greatly suffused, and the pupils contracted nearly to the size of a pin's head.

"His face, hands, and head, were bathed with warm vinegar and water, jars filled with hot water applied to his feet, and about two o'clock, p.m., he commenced taking tartar emetic in doses of a quarter of a grain every hour. It was combined with a small quantity of opium.

"When I saw him again, about nine o'clock in the evening, he was wonderfully improved. He could be easily roused, answered questions distinctly, put out his tongue when desired, and appeared quite rational. He had taken about two grains of the tartar emetic, the effects of which appeared to be chiefly confined to the circulating system. His pulse was now equal and regular, the temperature of his body nearly uniform, and a slight degree of moisture could be felt on his skin, but he was neither vomited nor purged. A mixture, containing nitrate of potash and tincture of hyoscyamus, was substituted for the tartar emetic; the fomentations of warm water and vinegar were continued, and he had a purgative enema with turpentine, which was followed by a full discharge from the bowels and copious diuresis. On Saturday, the 28th, he had an indistinct but favourable crisis; his tongue became clean and soft, and his pulse diminished in frequency. On the following Tuesday his pulse was 76, his tongue clean, eyes clear, pupils natural, appetite returning, so that I considered it unnecessary to continue my visits beyond the following day. His convalescence is now completely established.

"I have been particular in describing the cerebral symptoms in this case, as the patient's head was neither shaved, blistered, nor leeched. A portion of his hair was cut off with a scissors, and this was all that was done in addition to what I have mentioned. I attribute his recovery to the tartar emetic and opium, as under its use he recovered in a few hours from a state of stupor and coma, which otherwise must have speedily terminated in death, and I think this valuable remedy has additional claims to notice, if (as it would appear from Kinsela's case) it can be employed as a substitute for all the ordinary and expensive remedies used on such occasions—remedies which, in dispensary practice, and among a pauper population like ours, it is often difficult, and sometimes impossible to procure."

It is well known that delirium tremens requires very different modes of treatment, varying according to the constitution, strength, age, and habits of the patient. In the young and robust, more especially when it is produced directly by excessive drinking, it often assumes a form exceedingly resembling that of delirium arising from sudden congestion or inflammation of the brain or its membranes, and then demands strictly antiphlogistic measures, such as venesection, leeching, cold to the head, and very active cathartics. These remedies will often speedily arrest the progress of the disease. On the other hand, we most frequently meet with delirium tremens calling for a totally opposite plan; for when it occurs in the old, debilitated, and confirmed drunkard, who has been repeatedly subject to its attacks, we are obliged to exhibit opium from the very commencement, and that in large doses, combined with porter, punch, or some other cordial. These two form the extremes, between which there are many intermediate varieties, each requiring a special modification of practice.

Thus, some must be treated rather actively, on the antiphlogistic plan at first, and immediately afterwards opiates may be used with advantage; while in others, opiates cannot be given alone at any period of the disease, so prominently marked are the symptoms of cerebral congestion; and yet these cases cannot be cured without narcotics. How then are they to be exhibited? Do we possess any medicine capable of modifying and diminishing their injurious effects when given where cerebral congestion exists? Undoubtedly we do; tartar emetic will often accomplish this desirable object, and in delirium tremens the value of its combination with opium is recognised by every practitioner of experience. Tartar emetic, boldly exhibited, is often our sheet anchor in delirium tremens, especially when the evidence of active determination to the head is undoubted. Then tartar emetic alone, in repeated doses, often powerfully contributes to produce tranquillity and sleep; but there are other more mixed cases, where we cannot cure without adding opium, sometimes in larger, sometimes in smaller quantities, to the solution of tartar emetic; and so it is with the delirium and sleeplessness, so often met with in fever.

Every one is acquainted with the indications denoting the propriety of adopting the antiphlogistic practice when these symptoms make their appearance in the commencement of fever. Then the lancet, leeches, purgatives, cold applications to the head, and finally, repeated doses of tartar emetic, tend powerfully to reduce vascular action, and diminish the violence of symptoms depending on cerebral congestion and excitement. Here the lancet and tartar emetic are our best opiates, our best restoratives of tranquillity and sleep. As the fever progresses, and when we have arrived at a more advanced stage of the disease, when maculæ make their appearance on the skin, and symptoms of general debility, announcing the typhoid type, begin to predominate, then we must proceed with more caution, even though our patient is totally deprived of sleep and is violently delirious. The lancet cannot now be resorted to; leeches, indeed, may be applied, but their effects must be carefully watched, as the patient will not bear copious depletion of any sort; tartar emetic may, nevertheless, be still given boldly, and will be found to answer our expectations.

But, if we have to contend with want of sleep and delirium at a still more advanced period of fever, we now often recognise that very combination of symptoms—the union of general debility and cerebral congestion, which in certain varieties of delirium tremens we have seen so successfully treated with tartar emetic and opium. Who will refuse to acknowledge the similarity between these cases of fever delirium and many varieties of delirium tremens? Are there not in both the same tremor and subsultus of the extremities; the same trembling of the tongue when the patient endeavours to put it out; the same starting and sleeplessness; the same rambling delirium or incoherence, combined, nevertheless, with the power of answering rationally when spoken to; the same character of the mental wandering, for in both they are extremely apt to rave as if employed in their ordinary occupations, and as if surrounded with their usual associates; in short, can any greater resemblance exist between two diseases arising from the operation of remote causes so different? We need not, therefore, be surprised at finding the same treatment applicable to both.

LECTURE XVII.

THE USE OF TARTAR EMETIC AND OPIUM IN FEVER—*continued.*

In my last lecture I alluded to the peculiar narcotic power of the preparations of antimony, and dwelt on the benefits derived from a combination of antimonials with those medicines which are strictly termed narcotics. I told you in that lecture that the good effects of tartar emetic in delirium tremens seem to be totally independent of its action on the stomach; for we have witnessed those effects when it had not excited either nausea or vomiting. I referred also to many instances of delirium tremens, in which opium in every form had failed in procuring sleep, and where a combination of tartar emetic and laudanum had succeeded in tranquillizing the patient and producing sound, refreshing sleep. Bearing this important fact in mind, we shall proceed to a further examination of the circumstances which require the use of tartar emetic in fever.

There is a peculiar stage in one form of fever, and that exceedingly dangerous and threatening, in which I have derived most signal benefit from the use of this remedy. A patient, suppose, gets an attack of fever, he has all the ordinary symptoms, as thirst, restlessness, heat of skin, quick pulse, and headache. You are called in about the third or fourth day, and find that he has all the symptoms I have mentioned still present; his face is flushed, his head aching, his pulse from 100 to 110, but not remarkably strong; you find, also, that he has been sweating profusely from the commencement of his illness, but without any proportionate relief to his symptoms, and that he is restless and watchful. You are informed that his perspirations are so great that his linen has to be changed frequently in the day, and that, notwithstanding this, the pulse has not come down, the headache is undiminished, and the patient has become more and more sleepless. Here comes a very important practical question. How are you to treat such a case? The patient has no epigastric tenderness, no cough, no sign of local disease in either the thoracic or abdominal cavities; he has been purged, taken diaphoretics, and perhaps mercurials; every attention has been paid to regimen, ventilation, and cleanliness; but still he lies there in a state of undiminished febrile excitement, with persistent headache, quickness of pulse, and sleeplessness.

In such a case as this you have nothing to expect from sweating; it will never produce any relief. I was called some time back to see a young gentleman in fever, who was placed in similar circumstances to those which I have just detailed. It was about the sixth day of his fever, and I found him with a pulse of about 110, with considerable restlessness and headache, and was informed that he had perspired profusely from the commencement of his illness. On hinting the necessity of more active treatment than that which had been employed, his physicians appealed to the perspirations as decidedly contra-indicating depletion. They said that the profuse sweating pointed out the impropriety of active measures, and that it was a symptom which would be speedily followed by relief. I was convinced that they had taken a wrong view of the case, and stated as my opinion that nothing was to be expected from the perspirations; that when co-existing with a persistent febrile condition of the system, when accompanied by quick pulse, headache, and restlessness, perspirations always indicated the necessity of antiphlogistic measures, and in particular for the use of the lancet. I instanced the case of patients labouring under arthritis with profuse perspirations, which gave no relief, and said that it was well known that such cases were most successfully treated by a full bleeding from the arm. I accordingly stated that although the disease was of five or six days' standing, and the pulse not very strong, I would advise immediate bleeding. Sixteen ounces of blood were therefore abstracted, with some relief to the patient, and without increasing his debility; and it was then a question what further steps were to be taken.

The young gentleman had been actively purged; he had no cough or abdominal tenderness; his symptoms were headache, sweating, and sleeplessness; and to these nervous agitation had now become superadded. I proposed here what surprised my colleagues very much, and this was, to give our patient large doses of tartar emetic. They said the practice was very strange, his voice being occasionally loud and menacing, at other times low and muttering. His friends became exceedingly alarmed, and every remedy which art could suggest was tried:—his head was shaved and leeched until they could leech no longer; cold lotions were kept constantly applied with unremitting diligence, and he was purged freely and repeatedly.

At this period, that is to say, about the eleventh day of the fever, I was requested by this gentleman's medical friends to visit him. On examining the patient, I found that he was constantly making violent efforts to rise from his bed, and that he had a great deal of the expression of countenance which belongs to a maniacal patient. Under these circumstances, I advised the use of large doses of tartar emetic, in the mode already detailed, except that, in this case, in consequence of the violence of the delirium, I ordered the quantity prescribed for a dose to be taken every hour, instead of every second hour. The patient took about ten or twelve grains during the course of the night, and next day his delirium had almost completely subsided. Under the use of the remedy he became quite calm, fell into a sound sleep, and began to recover rapidly.

In the two preceding cases I was guided by ordinary principles, recognised by all physicians, and according to which the exhibition of tartar emetic is recommended in fever, wherever there is undoubted evidence of determination of blood to the head, producing headache, loss of sleep, and delirium. In the cases which follow, tartar emetic was exhibited at a period of fever, and under circumstances that were, with respect to the exhibition of this remedy, not less novel than important. The principles which led me to this practice have long been established, but, nevertheless, the practice is entirely new, and (I say it with pride, for it already has been the means of saving many valuable lives) it is entirely my own.

Shortly after the commencement of our present session, Mr. Cookson, a pupil at this hospital, and remarkable for his diligent attention to clinical pursuits, caught fever while attending our wards, in which many cases of the present epidemic were then under treatment. His fever was of an insidious nature, not characterized by any prominent symptom, not exhibiting any local disease to combat, or any tendency to crisis. For the first seven or eight days, with the exception of headache, which was much relieved by leeching, he seemed to be going on very well; his skin was not remarkably hot; he had no great thirst, nausea, or abdominal tenderness; his pulse was only 85; and he had sweating, which was followed by some relief. About the eighth or ninth day the pulse rose, and he began to exhibit symptoms of a hysteric character. Now, in every case of fever, where symptoms resembling those of hysteria come on, you should be apprehensive of danger. I do not recollect having ever met with a single case of this kind, which did not terminate in nervous symptoms of the most formidable nature.

I prescribed at the time the usual anti-hysteric medicines, but without any hope of doing good, knowing that these symptoms were only precursory to something worse. I also, as a precautionary measure, had leeches applied to his head. The fever went on, the headache became more intense; he grew nervous and sleepless, and fell into a state of great debility. On the fourteenth day of fever his tongue was black and parched, his belly tympanitic; he was passing everything under him unconsciously; he had been raving for the last four days, constantly attempting to get out of bed, and had not slept a single hour for five days and nights.

Dr. Stokes, with his usual kindness, gave me the benefit of his advice and assistance at this stage of Mr. Cookson's illness, and we tried every remedy which experience could suggest. Blisters were applied to the nape of the neck, the head was kept cool by refrigerant lotions, the state of the belly attended to, and, as we perceived that the absence of sleep was a most prominent and distressing symptom, we were induced to venture on the cautious use of opium. It was first given in the form of Dover's powder, with hydrargyrum cum cretâ, with the view of relieving the abdominal symptoms as well as procuring sleep. This failing in producing the desired effect, we gave opium in the form of enema, knowing its great power in the delirium which follows wounds and other injuries. This was equally unsuccessful with the former. He still was perfectly sleepless. We came again in the evening, and, as a last resource, prescribed a full dose of black drop, and left him with the conviction that if this failed he had no chance of life.

On visiting him next morning at an early hour, we were highly mortified to find that our prescription had been completely unsuccessful; he had been more restless and delirious than ever. Here was the state in which we found him on entering his chamber at eight o'clock in the morning of the fifteenth day of his fever. He had universal tremors and subsultus tendinum, his eye was suffused and restless, he had been lying for some days entirely on his back, his tongue was dry and black, his belly tympanitic, his pulse 140, quick and thready, his delirium was chiefly exhibited in short broken sentences, and in a subdued tone of voice; and it was now eight days and nights since he had slept. Here arose a question of great practical importance. How was the nervous agitation to be calmed and sleep procured? Blisters to the nape of the neck, cold applications, and purgatives had failed, opium in various forms had been tried without the slightest benefit; if sleep were not speedily obtained he was lost.

At this emergency a mode of giving opium occurred to me which I had never thought of before. Recollect what his symptoms were at this period, quick, failing pulse, black, dry, tremulous tongue, great tympanitis, excessive prostration of strength, subsultus tendinum, extreme nervous agitation, constant muttering, low delirium, and total sleeplessness. I said to Dr. Stokes that I wished to try what effects might result from a combination of tartar emetic and opium; I mentioned that I had given it in cases of delirium tremens with remarkable success, and thought it worthy of trial under the circumstances then present. Dr. Stokes stated in reply, that he knew nothing with respect to such a combination as adapted to the case in question, that he had no experience to guide him, but that he would yield to my suggestion. We therefore prescribed a combination of tartar emetic and laudanum in the following form, which is that in which I generally employ the remedies in the treatment of delirium tremens: tartar emetic, four grains; tincture of opium, a drachm; camphor mixture, eight ounces; mix. Of this mixture, a tablespoonful to be taken every second hour. The success of this was almost magical. It is true that it vomited him; after taking the second dose he threw up a large quantity of bile, but it did him no harm. After the third or fourth dose he fell asleep, and awoke calm and refreshed. He began to improve rapidly, and soon recovered.

The next case to which I shall direct your attention is that of

Mr. Stephenson, a pupil of Mr. Parr of this hospital. This young gentleman, as many of you may recollect, was attacked with fever about the middle of January. On Thursday evening he complained of languor and malaise, and on the following day felt himself feverish, but without any prominent or decided symptom. At night he took a dose of calomel and antimonial powder, which had no sensible effect, and the following day complained of shivering, violent headache, pain in the back, thirst, prostration of strength, and sleeplessness. He was ordered to take a combination of tartar emetic and nitrate of potash in camphor mixture, which produced a few loose stools and some diaphoresis; but in consequence of its effect on the stomach, and his complaining much of thirst and epigastric tenderness, the tartar emetic was omitted, and effervescing draughts prescribed. Two days afterwards, the epigastric tenderness still continuing, twelve leeches were applied over the pit of the stomach, followed by a blister which gave relief, and the bowels were kept open by enemata.

He commenced a second time the use of the tartar emetic and nitrate of potash, with the addition of five drops of tincture of opium to each dose, but was obliged to give it up again in consequence of the increase in his gastric symptoms. He now became exceedingly restless, and his delirium began to assume an intense character. Leeches were applied behind the ears, his head shaved, and his temples blistered; he had also a large blister over the abdomen, which gave him considerable relief, but the cerebral and nervous symptoms became much worse. The delirium went on increasing, accompanied by subsultus tendinum, and picking the bed-clothes; he was perfectly sleepless, raved incessantly, and had to be kept down in bed by force. On the seventeenth day of his fever he was in the following condition:—tongue brown and rather dry, no remarkable thirst nor abdominal tenderness, eyes red and ferrety, no sleep for five nights, constant muttering and delirium (which had now assumed the character of delirium tremens), subsultus tendinum and jactitation extreme, urine and feces passed under him unconsciously. I directed the combination of tartar emetic and laudanum to be immediately given, carefully watching its effects. He had only taken two doses when a degree of calmness set in, bringing with it relief to all his symptoms, and before a third dose could be administered, he fell into a profound sleep, from which he awoke rational and refreshed. The mixture was continued every four hours with increasing benefit, he slept long and soundly, and began to improve in every respect. On the second day after he had begun to use the tartar emetic, he took a little porter, which was changed the next day for claret and chicken broth. In about a week he was able to sit up in bed, and seven days afterwards was able to leave the hospital and go to the country for change of air.

Another case to which I shall direct your attention is that of Mr. Knott, also a pupil of this hospital, a gentleman remarkable for his unremitting attention to clinical pursuits, and from whom I derived much valuable assistance in conducting various post-mortem examinations. This gentleman was attacked with fever about the latter part of January, which went on for some time without any particular symptom, except considerable restlessness and nervous excitement. He then became perfectly sleepless, complained of violent headache and thirst, raved, and became exceedingly irritable. Opium, in various forms and repeated doses, either alone or combined with musk and camphor, totally failed in producing sleep, and his condition became daily worse. On the thirteenth day he was in a very dangerous condition; his nervous agitation had risen to an alarming height, and for many days and nights he had never closed an eye. At this period it appeared obvious that if something were not done to calm nervous excitement and restore sleep, he had but little chance of life. Under these circumstances I proposed to Dr. M'Adam, who attended with me, to give tartar emetic and opium. After he had taken about three tablespoonfuls, he had a copious bilious evacuation, and immediately afterwards fell into a sound sleep, during which he perspired profusely, and awoke in about twelve hours with every bad symptom gone. The nervous irritability was completely allayed; his thirst and headache relieved; his tongue moist and cleaning; and his reason quite restored. From that period everything went on favourably, and he rapidly gained his health and strength.

In many other cases of fever I have recently employed the tartar emetic and opium with the same remarkable success. A man named Christopher Nowlan was admitted into Sir Patrick Dun's Hospital, on the 3rd of February, labouring under fever.

He had been ill ten days, had raving, subsultus tendinum, and appeared unable or unwilling to answer questions. His wife stated that he had diarrhœa for the preceding three days, and that he dozed occasionally, but never slept. He appeared exceedingly low and prostrated, and lay constantly on his back. A succession of flying blisters was ordered to be applied to the chest and stomach, and wine and chicken broth prescribed. He also got the following draught every third hour:—

℞. Misturæ Camphoræ, f℥j.
Spiritus Ætheris oleosis, fʒss.
Spiritus Ammoniæ aromatici, fʒss.
Moschi, gr. viij.—Misce.

Under the use of these remedies he began to recover from his prostration; but as the sleeplessness and delirium still continued, I ordered him to take the tartar emetic mixture in the usual way. It produced at first two or three full discharges from the bowels, and after he had taken the fourth dose he fell into a sound sleep, from which he awoke much better, and soon became convalescent.

In the case of a patient named Michael Murray, who exhibited the same remarkable nervous irritability and sleeplessness, this remedy was also employed with very striking effects. This man had been ill of fever for ten days before his admission into Sir Patrick Dun's Hospital, and appeared so much prostrated that I ordered him arrowroot with beer. He raved a little on the night of his admission, and remained without closing an eye until morning. The same symptoms were observed on the following day, and his nervous irritability became increased. On the 14th of February he had been five days in the hospital, and had not enjoyed a single hour's sleep. I ordered the tartar emetic mixture to be given: three doses produced sleep; he had no other bad symptoms, and recovered completely.

In another very bad case of maculated fever, the same results were obtained. The patient, Mary Farmin, had got an attack of fever after a fright. She had been eight days ill at the date of her admission, February 25th. She had irregular pulse, sleeplessness, headache, and suffusion of the eyes; moaned and sighed continually, and appeared greatly prostrated. She was blistered, had fœtid enemata, and took the chloride of soda internally with some benefit; but the sleeplessness and nervous excitement continued. In this case, though the tartar emetic was not followed by speedy convalescence, still it produced remarkably good effects; after taking four doses of it, she fell asleep, and did not awake until next morning.

Several other cases have occurred both in hospital and private practice, to some of which I now beg leave to direct your attention, observing that I have in every instance been particular in mentioning the names of other professional gentlemen who witnessed the progress of each case: a precaution tending to prevent exaggeration either in detailing symptoms or describing the effects of remedies.

The case of Mr. William Murphy, an extremely diligent and intelligent pupil at the Meath Hospital, is well worthy of notice. The father of this gentleman, a practitioner of well known reputation at Fermoy, where he has been Physician to the Fever Hospital for many years, arrived in Dublin the very day his son's state appeared to be hopeless, soon after the consultation, when Dr. Stokes and I agreed to use the tartar emetic and opium; Dr. Murphy admitted afterwards that he never felt so much surprised as he was at this treatment, but having entrusted the care of his son to us, he very properly expressed no opinion on the subject, a mode of proceeding he has never since ceased to congratulate himself on, for had he opposed us, the case was apparently so desperate, that it may be doubted whether we would have ventured to put the plan into execution.

Mr. Murphy, aged 20, having been engaged in the diligent study of the fever cases in the Meath Hospital, was attacked with violent symptoms of fever on the 6th of January last. He took a dose of calomel and James's powder, and went to bed; early next morning he was worse, and although he took a purgative draught which operated freely on the bowels, he complained much of headache, and was very feverish; a copious sweat broke out, but was unattended with relief, notwithstanding that it continued with more or less interruption for several days. His thirst was excessive, and he was very restless, depressed, weak, and nervous; the antimonial powder and calomel were persevered in during the second day, and on the third he took more purgative mixture, and twelve leeches were applied to the temples, but they gave little or no relief to the pain in the head.

In short, he grew worse, and was found to be extremely prostrated. On the fourth, his tongue was foul and dry, his stomach irritable, often rejecting his medicine, and producing a vomiting of bilious matter, the pulse quick, and his appearance unpromising. I saw him on the fifth day, when everything was still worse, and the pain of head much complained of. I directed a continuation of the James's powder, and effervescing draughts. On the sixth day he was still worse, and was reported to have raved a good deal during the night; his bowels were loose, and now for the first time the perspiration entirely ceased, and his skin became hot and dry. I gave him small doses of Dover's powder and chalk. On the seventh day, his countenance expressed great anxiety, and in addition to an aggravation of all the other symptoms, his skin became covered with a measles-like eruption of maculæ, a circumstance which induced me to give the solution of chloride of soda, in doses of twelve drops every fourth hour, in an ounce of camphor mixture. He got mild diet, as arrowroot and chicken broth, with a little stale bread sopped in tea, night and morning. On the eighth day, no improvement; much raving during the night, symptoms as before, except that the occurrence of some abdominal tympanitis and slight epigastric tenderness induced me to apply six leeches to the pit of the stomach. The bleeding from the leech-bites was moderate, but seemed, nevertheless, to exhaust him. It seemed to check the tympanitic tendency. On the ninth day, was still worse, much stupor, incipient subsultus; towards evening a very hurried and laboured breathing supervened, and he lay entirely on his back, helpless and weak, respiring about 45 times in a minute. As he had not the slightest affection of the lungs or bronchial tubes, this hurried breathing excited the greatest alarm in my mind, and induced me to apply six leeches behind the ear, with a view of relieving the now increasing stupor, and the evident cerebral congestion.

On the tenth day, I had the benefit of Dr. Stokes' advice. We found our patient in a state truly appalling. He lay panting on his back, restless and without sleep, every muscular fibre in his face and limbs was agitated with spasmodic twitches, giving rise to the greatest possible degree of subsultus, which distorted his face, caused him to bite his under lip every instant, rendered him quite unable to put out his tongue, although he endeavoured to do so. The subsultus prevented us from being able to feel the pulse, now weak and rapid, at the wrist. In the meantime, though he often moaned and raved, he muttered indistinctly; he evidently understood what was said to him, and as far as we could collect, he seemed to suffer much less from pain in his head. Still the temporal arteries were turgid, and his eyes suffused. He had retention of urine, and since yesterday it was drawn off with the catheter.

What was now to be done? Cold lotions to the shaved head had failed—a blister to the nape of the neck had proved useless—we could not venture to rely on more blistering of the scalp—some more powerful remedy must be instantly brought to bear, or our patient was lost. Alvine evacuations had been pushed to the fullest extent; leeches could not even be proposed, so great was the debility. Opium we dared not venture on, seeing that so recently the pain in his head had been urgent, and that the temporal arteries and the conjunctiva still seemed to indicate cerebral congestion; under these circumstances we resolved to try tartar emetic, and we ordered the following mixture:—

℞. Tartari Emetici, gr. ij.
Moschi, gr. xxx.
Mucilaginis,
Syrupi simplicis, āā. f℥j.
Aquæ, f℥x.
Misce, sumat ℥ss. omni horâ.

After he had taken about six doses of this medicine, he seemed rather better, and the symptoms of determination to the head appeared less marked; we therefore added fifteen minims of patent black drop to the remaining nine ounces of the mixture, and directed small quantities of porter and chicken broth to be given repeatedly during the night. On the eleventh day we found a change for the better truly surprising, the pulse had diminished remarkably in frequency, and had become softer and fuller; a warm sweat had broken out, he had raved but little, and had slept tranquilly. We ordered a continuance of the same nourishment and medicines, the latter at much longer intervals; the case need not further be detailed, as Mr. Murphy rapidly recovered, and enjoyed a speedy convalescence. Here then is a case which would assuredly have been lost but for the well-tried application of the new method of treatment. I say this emphatically, for Mr. Glyssan, Mr. Boyton, Mr. Clarke, and Dr. Murphy, all anxious and competent observers, assured us that from the moment he began the bottle, its good effects were apparent, and increased after each dose.

The next case I shall mention is that of John Doyle, admitted into the Meath Hospital, May 21st, 1835; three or four days ill, a strong young man; the symptoms were attended with considerable reaction at the beginning, his face being flushed, eyes wild, and head aching; he raved much during the night from the fourth day, and had then a full bounding pulse at 105. Venesection was ordered, but he fainted when four ounces of blood had been drawn. Leeches were then applied to the epigastrium. On the sixth day of his illness, his thirst was great, no sleep, skin moist, belly soft, pulse 120, pain in head severe, copious eruption of maculæ. His head was now shaved, and six leeches applied behind the ear, and repeated three times. He was ordered the liquor of the chloride of soda on the seventh day, as the vascular excitement had then diminished, and the maculæ constituted a prominent feature in his case. On the eighth day he was not worse, but his skin was very hot. On the ninth day, eyes suffused, face flushed, much thirst, no sleep, bowels free, belly soft, some epigastric tenderness, tongue loaded, but moist: cold lotions to the head. Tenth day, delirium violent during the night, strait waistcoat necessary, eyes suffused, belly soft, skin very hot, pulse 120, respirations 40, considerable subsultus. Six leeches to be applied behind the ear three times successively.

R. Tartari Emetici, gr. iv.
Aquæ, f℥xvj. Misce, sumat semiunciam omni horâ.

Eleventh, slept very little, delirium less violent, one very large stool, heat of skin less, eruption copious.

R. Misturæ Camphoræ, f℥viij.
Tartari Emetici, gr. iv.
Tincturæ Opii, fʒj.
Misce, sumat ℥ss. secundâ quâque horâ.

Twelfth, slept five hours, seems better, but still he passes his stools under him; pulse 120, eyes suffused, skin hot, tongue cleaning, belly soft, bowels loose, maculæ numerous. The same prescription, except that the tincture of opium was increased to ʒiss. in the eight ounce mixture.

Thirteenth, the medicine was continued for several hours, when he fell asleep, and slept so much and so tranquilly, that it was not thought necessary to repeat it. Pulse 110; subsultus not near so violent; does not rave; knows every one, and answers rationally; light nourishment.

Fourteenth and fifteenth, improvement continues, but still there is much fever, and many maculæ. About the twenty-first day he was free from fever, but he got no medicine after the night of the twelfth.

This case exemplifies the treatment adapted to the three different stages—1st, Bleeding, leeches, cold lotions: 2nd, Tartar emetic in large doses, combined with leeching: 3rd, Opium boldly administered in combination with tartar emetic.

The following, communicated by Mr. Knott, excited much interest among the practitioners of the neighbourhood:—

"On the 20th of July last, I was called to see a comfortable farmer, residing near Boyle, in the county Roscommon, named J. K. He was aged 30 years, and had been ill twenty-one days. His fever commenced with rigor, headache, and pains in the loins, the headache being particularly severe. In the commencement of the fever he had raved incessantly; slept but little; had frequent retching; his bowels were confined. For these symptoms, he was purged with black bottle to excess, and bled largely and frequently, but without any permanent alleviation. On the twenty-first day of his fever he presented the following appearance and symptoms:—his countenance was expressive of great anxiety and ferocity; his eyes were bloodshot and wild; teeth covered with sordes; tongue brown and furrowed with clefts; he raved violently, and attempted to get out of the bed several times; great excitement and subsultus; his skin was very hot and dry; all the secretions much diminished; urine high coloured; no eruption; no epigastric tenderness; abdomen slightly swollen and tympanitic, but pressure seemed to give no pain; his bowels had not been open for three days. That night he was ordered 40 drops of the tincture of opium, at the same time that an enema was exhibited; the bowels were once opened; he slept none during the night, and the excitement was, if anything, greater than before. Under these circumstances it was wiry than on yesterday; epigastric tenderness much relieved, headache gone, maculæ less.

To have a pint of beer and arrowroot.

R. Solutionis Chloridi Sodæ, min. xv.
Misturæ Camphoræ, f℥j.
Guttæ nigræ, min. j.
Misce, fiat haustus quater in die sumendus.

10th.—Raved the whole night; subsultus general and violent; pulse 120, sharp; slightly dicrotous; slept none; face much more flushed than on yesterday; eyes suffused; passes under her; maculæ much diminished; has no headache; bowels rather free; lies on her back with her feet drawn up; has no chest symptoms; respiration natural; ordered ice in bladders to the head; with a mixture composed as follows:—

R. Misturæ Camphoræ, f℥viij.
Tartari Emetici, gr. j.
Misce, sumat f℥ss. omni semihorâ.

11th.—When seen yesterday evening she was very violent; endeavoured to get out of bed; screamed loudly, and complained of bad treatment; she had slept none at this period, her bowels had been freed copiously, but she still continues to pass under her; she endeavours to throw the ice bags off her head, and requires some violence to hold her in bed; subsultus extremely violent; face much flushed; eyes red; she was ordered the following:—

R. Misturæ Camphoræ, f℥viij.
Tartari Emetici, gr. iv.
Tincturæ Opii, fʒj.
Misce, sumat f℥ss. secundis horis.

She had taken but two tablespoonfuls when she began to sleep; she has continued to doze to the hour of visit; she is much improved in every respect; she answers questions rationally; her face is not so much flushed; eyes less suffused; has no headache; pulse 120, not so sharp; skin still very hot; tongue moist and cleaning. She was ordered not to take any of the mixture if she continues better. Enema emolliens statim. Improvement went on steadily until convalescence was established.

The next case I read from the report of Dr. Dwyer, who was the physician in attendance:—"In compliance with your request, I send you an abstract of the case of Stephens. It was one of spotted fever occurring in a young man of temperate habits, setting in with languor followed by rigor. I saw him on the fourth day, when there was unpleasant heat of surface, with general tenderness all over the body, particularly remarkable over the epigastric region; the chest, arms, and hands studded with florid maculæ; headache and pain of back distressing; light disagreeable; pulse 108; tongue moist. He had an oil draught, followed by small doses of hydrargyrum cum cretâ with Dover's powder. On the sixth day of his fever, being very restless and sleepless, eyes slightly suffused, and pulse 120, I gave him an eight ounce mixture, containing four grains of tartar emetic, and a drachm of tincture of opium; two tablespoonfuls to be taken in the evening, and one every hour afterwards. On the next day the report was, that he had slept a good deal during the night, having fallen asleep after the third dose, three hours after which a fourth was administered. He is dozing, pulse 120, skin hot and dry, bowels four times moved; ordered to continue his mixture; watching its effects. On the eighth day, in consequence of severe purging having set in (he had taken but two doses of the mixture since last report), the epigastrium becoming very tender, and pulse 132, his medicine was omitted and a cretaceous mixture ordered instead, a small quantity of port wine diluted, and a blister to the abdomen; the blister was not applied, yet the purging was checked. On the evening of the ninth day, as he complained much of want of rest, and there was no headache, I directed him to have two doses of the tartar emetic and opium mixture, within an interval of two hours.

"I was compelled at this period to give up attendance on this case in consequence of an accident; it was, however, taken up by Dr. Grant, who kindly kept notes, and with whom I had daily conferences. He reports our patient on the tenth day to have suffered an accession of fever, seemingly caused by abdominal irritation; he complained much of headache; the eyes were injected; skin hot and dry; tongue brown and crisp; pulse 144; respiration 49; throbbing of the temporal arteries; when undisturbed, raving and moaning, but answers rationally; abdomen full and tense, tenderness in region of colon, with some tenesmus;

sleeplessness. He was given four grains of calomel and three of extract of hyoscyamus, followed by an oil draught; a blister was applied to the abdomen, cold to the head, and warmth to the feet. The medicine acted well, producing a number of dark-coloured motions, with some relief of the symptoms; the sleeplessness, however, still continuing. On the twelfth, raved considerably the previous night, with great restlessness; headache with darting pain; pulse 120; still answers rationally, but raves when left to himself; abdomen soft; he was again put on the use of the tartar emetic and opium mixture, to have one tablespoonful every hour for three doses, and then only every second hour. On the following day there was a considerable improvement; he had slept well, and perspired freely in the night; no raving; headache had gone; pulse 96; heat of skin less; to continue his mixture. On the fourteenth day he was much better; he wished for food. On the fifteenth he suffered a relapse, from his appetite having been imprudently indulged; he was given an oil draught, and directed to resume his mixture when the bowels acted. He continued from this time to improve, the interval between the doses of his mixture was gradually lengthened, and on the seventeenth day he was convalescent.

"In this case the good effects of this mixture were evidenced by perspiration and rest. This lad's mother and sister were just convalescent from spotted fever; the former four weeks, the latter a fortnight. In the mother's case I was not applied to till the tenth day; it went on till the twenty-first. There was not any organ particularly implicated; she was treated with stimulants, carbonate of ammonia, porter, and blisters. In the daughter the fever was very severe to the eleventh day, when it terminated by profuse perspiration. She suffered principally from pain in her head and back, with intolerance of light, and was treated with mild aperients, followed by diaphoretics with hyoscyamus. In neither was sleeplessness distressingly remarkable. Another brother was seized with the same form of fever a few days after the subject of this case had taken ill; he was on the fifth day transferred to Sir Patrick Dun's Hospital.

"I experienced marked benefit from this form of prescription in a case of melancholia, occurring in a female aged 45, consequent on a severe domestic affliction. The exhibition of it

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here, however, was followed by considerable debility, requiring stimulants. This effect I consider to have been, in some degree at least, attributable to the patient having for some days previous to its exhibition refused food, and possibly been suffered to remain too long under the sedative influence of this medicine without having been offered nourishment."

The following is also an interesting example of the efficacy of this plan of treatment:—

John Dillon, aged 15, a servant, admitted into hospital, 5th June, 1835, several days ill. On the day of his admission he had headache, thirst, heat of skin, loss of appetite and rest; his face was flushed and bloated; eyes suffused, red and prominent; skin hot and dry. He complained of slight epigastric tenderness and violent headache; pulse 120, full and bounding. His whole body was covered with maculæ; bowels regular, tongue brown, furred, and dry. Ordered

> ℞. Aquæ, f℥j.
> Liquoris Chloridi Sodæ, min. x.
> Misce, fiat haustus quartis horis sumendus.
> Applicentur hirudines xii. post aurem, et repetatur applicatio si opus sit.

7th.—The leeches bled freely; head appears to be relieved; he raved a good deal during the night; his pulse has fallen to 100, but still very full; has a slight cough, and some bronchitis. Ordered to repeat the draught, and apply four leeches to the larynx.

8th.—Slept very little; does not appear improved; very irritable; raved, and was rather violent during the night; cough better; tongue very brown and dry; bowels confined; pulse 100; respirations rather hurried. Ordered to repeat the draught, and to have an emollient enema in the evening.

9th.—Epigastric tenderness much increased; raved continually during the night; slight subsultus; eyes very red, wild and staring; pulse 114, very full; tongue dry and brown; teeth covered with sordes. To repeat the draughts, and apply eight leeches to the epigastrium.

10th.—Appears better to-day; epigastric tenderness much relieved by the leeching; his strength is much prostrated;

wishes for more food; pulse 100, and still full; slept none. Ordered arrowroot, and to repeat the draughts.

11th.—The fever is again much increased; raved violently during the night; great prostration; slept none; subsultus very violent; great thirst; pulse 130; complains of a heaviness, but no pain in head; skin very hot and dry; eruption undiminished. Ordered to repeat as before.

12th.—All the symptoms much aggravated; face flushed and red; eyes suffused and ferrety; teeth covered with sordes; lips parched and cracked; tongue black and very dry; subsultus general and violent; does not sleep either by night or day; exceedingly irritable; pulse 130 and jerking; pupils contracted; he lies on his back with legs drawn up; extremities rather cold. He was ordered warm applications to his feet and the following prescription:—

℞. Tartari Emetici, gr. ij.
Misturæ Camphoræ, f℥viij.
Tincturæ Opii, f℈ij.
Misce, sumat cochleare unum amplum secundâ quâque horá.

13th.—The nurse reported that after he had taken the mixture three times, he slept calmly for nine or ten hours, the first time for the last week. It operated largely after the second dose, the stools being thin and bilious. He has ceased to rave; the suffusion has quite disappeared; tongue is moist and cleaning. He slumbers continually; subsultus completely subdued; answers questions rationally; pulse has fallen to 98 and soft; ordered to repeat the mixture.

14th.—Slept continually since last report; general appearance much improved; perspired profusely during the night. He was perfectly sensible from this day till the 17th. He continued to improve rapidly in strength and appearance.

17th.—Convalescent.

And with one more case I shall conclude for to-day. Mr. S., residing in College, was attacked with headache on the 3rd February, 1836, and fever commenced on that or the following day. He was judiciously treated by Mr. Barker, of Britain Street, until the fourth day of the fever, when an increase of headache and pain in or behind the ball of the right eye induced him to call me in. A bleeding from the arm much relieved the pain, and he spent a tranquil night. He got calomel and James's powder in small doses. On the fifth no change. Sixth day of fever, maculæ began to appear, and his state became more alarming. Seventh day, maculæ abundant, restlessness, debility, very frequent sighing, thirst, &c., with a sharp pulse, and return of headache. Leeches to head and nostrils were ordered; the latter because of an evident tendency to epistaxis. Eighth, Sir Henry Marsh saw him along with us. Ninth and tenth, grain doses of Dover's powder added to his medicine four times in the night, but did not procure rest.

Eleventh, perfectly sleepless night and day; ordered in the evening one grain of tartar emetic, four ounces of camphor mixture, and one scruple of laudanum: one tablespoonful every second hour. Twelfth, moisture on skin; began to sleep after second dose, and slept several hours tranquilly; is to-day quite free from muttering and raving, which had commenced on the tenth day, and increased on the eleventh; so that when left to himself he lay on his back, constantly speaking, but not in a loud or boisterous manner, his eyes being all the time open; when addressed he answered quite rationally, but on our quitting the room began again immediately to ramble. This group of unpleasant symptoms having disappeared, we did not continue the medicine, but ordered palliatives and mild nourishment; in the evening it was judged right to apply a blister to the nape of the neck. Thirteenth day, maculæ abundant; was quiet during the night, but did not sleep at all; exhausted and nervous; other symptoms moderate; pulse 104; tongue moist; abdomen a little swollen and slightly tympanitic; turpentine injections; palliative diuretic draughts; chicken broth; claret and water. At five p.m. I again saw him, and found him still quite sleepless, but without headache; bowels moved, but still slightly tympanitic. Fearing the continued exhaustion from want of rest, I now ordered a mixture consisting of one ounce of mucilage of gum arabic, seven ounces of camphor mixture, three grains of tartar emetic, and one drachm by measure of laudanum; half an ounce every second hour, until sleep comes on.

At ten, Sir Henry Marsh and Mr. Barker saw him; he had slept an hour; appeared drowsy, and did not complain of headache; two doses of the medicine had been given; he remained awake until eleven, when another dose caused him to sleep until three; at four another was given, after which he slept until eight, and awoke much refreshed, and much improved in every respect; his belly had not been moved, and was still slightly tympanitic, a symptom which yielded to the administration of two drachms of castor oil exhibited in the form of an aromatic emulsion. In the evening he was ordered to take four drops of black drop, but this procured no sleep during the night. On the morning of the fifteenth day we found him somewhat exhausted from a sleepless night, but with much less fever and no headache: pulse 94, soft; for the first time we remarked subsultus: a family idiosyncrasy, rendering musk peculiarly disagreeable, or even intolerable, we ordered a draught containing two drops of black drop, and fifteen of Hoffman's liquor, every fourth hour. In the evening he had slept very little, so that I resolved again to recur to the antimonial opiate, two spoonfuls of which produced sound refreshing sleep for several hours. In the morning he again got castor oil; and on this, the sixteenth day, his pulse was only 70; but still, though the subsultus was diminished, a remnant of it could be perceived, so that he could not be pronounced out of all danger.

The conclusion of this case is peculiarly instructive, and proves how insidious is the progress of fever, and how unsafe the condition of a patient, whose brain and nervous system have received a violent shock, even although the immediate consequences of that shock have been averted by the employment of decided treatment. On the sixteenth day we have seen an abatement, or rather a disappearance of almost every symptom of the disease, save and except a slight, a scarcely perceptible remnant of the subsultus. Great care was taken to prevent his being disturbed, and the strictest attention as to diet was enjoined; indeed he was remarkably disinclined to taking food, and it was with great difficulty that we could get him to consume a sufficient quantity of mild farinaceous diet. On the night of the sixteenth day he slept tolerably. The seventeenth day was passed without any change; but he slept none that night.

The eighteenth day he was perfectly free from fever; pulse 70; tongue moist; bowels opened by medicine. That day he conversed too much to his friends about his removal to the country, his future plans, &c.; but nevertheless he slept several hours towards evening. This sleep was disturbed and chequered by dreams, and on awaking about eleven o'clock, he was wandering, and got eight drops of black drop, which procured no rest; on the contrary he got several times out of bed, and spoke incoherently. The raving had all subsided at 10 a.m. on the nineteenth day, when I was in hopes it was entirely owing to temporary excitement, and would not return; an opinion rendered probable by a total absence of all symptoms of general or local vascular excitement, of headache, &c. In this expectation, however, I was disappointed, for early in the afternoon he became incoherent; raved more and more every hour; complained of headache; could not bear the light; and when I saw him at seven, he was quite irrational; supposed himself to be travelling; and when questioned he seemed not to understand; his pulse had fallen below 60; was soft, irregular, and intermitted very frequently; skin not hot; feet cold; features contracted; tip of nose cold; he had eaten stirabout in small quantity twice during the day, but in a voracious unnatural manner; his eyes were a little red, and everything wore a most threatening aspect.

What was now to be done? In directing his head to be shaved anew, and in applying blisters to his scalp and temples, I felt I was proceeding on sure grounds; but the indications for the internal treatment were less obvious. We had arrived at the nineteenth day, and he had gone through a debilitating fever, and had been submitted to a very active mode of treatment. Were we to leech the head? were we to apply cold? and should we immediately endeavour to mercurialize the system by means of mercurial preparations, given internally and applied externally? Such would have been the treatment a patient, under similar circumstances, would have undergone at the hands of any practitioner a very few years ago; and I have no doubt that a treatment of this nature would have speedily brought matters to a fatal termination.

The writings of Gooch, however, who pointed out the diagnosis and treatment of certain cases, usually confounded with inflammatory hydrocephalus, and the influence of the truth of Dr. Gooch's statement, as illustrated by several examples in our own practice, determined Sir Henry Marsh, Mr. Barker, and myself to rely on the severe blistering locally, while internally we ordered a draught consisting of two grains of carbonate of ammonia, twenty drops of Hoffman's liquor, and one ounce of camphor mixture, to be taken every third hour. Warmth was applied to the feet, and he was supplied with warm whey. Shortly after our visit he fell asleep, slept with little interruption for about seven hours, and awoke perfectly rational; and at eight o'clock next morning, being the twentieth day, we found him much better in every respect; the only vestige of this alarming attack that remained being some intermission in the pulse, which had become in other respects much more natural and fuller. The bowels had not been opened; a circumstance I mention because, no doubt, some would have ordered purgatives on such an emergency, a practice which the fallen, soft state of the belly did not seem to us to call for, and which our view of the nature of the case prevented us from proposing. We ordered farinaceous diet, and a repetition of the draughts, at longer intervals. In the evening of the twenty-first day the pulse had lost all remnant of irregularity or intermission, and the disturbance of the nervous system had entirely subsided: from that period his convalescence commenced.

One fact connected with the cases just related is very striking, viz., the small quantity of laudanum which, in most of them, was sufficient to induce sleep; a circumstance only to be accounted for by the presence of the tartar emetic, which no doubt exerts, when given in duly regulated doses, a powerfully tranquillizing effect on the nervous system. It is also deserving of remark, that the combination very seldom gives rise to any of the unpleasant symptoms that so frequently arise when opium alone, or any of its preparations, are given with a view of producing sleep at an advanced period of fever. The addition of one ounce of mucilage, and one ounce of simple syrup to the mixture, seems to render it less likely to disagree with the stomach. Towards the termination of fever, it not unfrequently happens that a sudden or gradual determination of blood to the head arises, and which requires a repetition of a modified system of antiphlogistic treatment, aided by blisters. This state, I have reason to believe, may be often prevented from occurring by a timely attention to procuring sleep; for a patient in fever, who has passed several sleepless nights, is on the verge of cerebral congestion or inflammation, as is testified by headache, wandering, and the redness of the conjunctiva. Here it is that the treatment I recommend is so advantageous, when timely applied;

sleeplessness. How many theories of fever were refuted by such a case! Usually, as the disease continued, and when the patient was in a very dangerous state—but seldom or never before that—the intestines began to be inflated, and the belly gradually became tympanitic; a circumstance of bad omen, and which was often the precursor of hiccup.

When the symptoms did not yield to the efforts of nature or art, the congestion of the intestinal mucous membrane, indicated by these symptoms, was soon followed by indubitable evidence of cerebral congestion—such as restlessness, suffusion of the adnata, and contraction of the pupils; this last was the most fatal of all symptoms. In two or three cases—as, for instance, that of Mr. Cookson—the cerebral congestion produced repeated fits of convulsions on the thirteenth day, and yet he recovered. The same happened in a young woman in Sir P. Dun's Hospital, in whom the convulsions occurred on the fifteenth day, and were more violent on the right side than on the left, producing strabismus, and insensibility of the pupil of the affected eye. This girl lost the use of her left side on that day, but recovered it on the following; and eventually, though with difficulty, was completely cured. Frequent fits of convulsions, affecting the right side more than the left, took place on the seventh day in the daughter of a clergyman residing in the Liberty, and were followed by a stupor bordering on coma, which lasted for many hours. All these patients were covered with maculæ.

There is one circumstance connected with this epidemic, which I have also frequently witnessed in other sporadic and epidemic fevers, to which I wish forcibly to draw your attention; it is the existence of tenderness generally over the body; and which causes the patient to shrink from the pressure of the finger, applied to any part of the integuments. This tenderness arises from an irritated state of the nervous system generally, and is usually accompanied by severe dorsal or lumbar pain, indicating spinal congestion. Now, in a practical point of view, this tenderness requires attention; for if it be overlooked, and if the physician applies pressure, in such cases, only to the epigastrium, he will be deceived into the belief that the tenderness he there discovers is confined to that part, and indicates the application of leeches to the pit of the stomach.

I am thus particular in dwelling on the symptoms manifestly denoting a combination of primary general nervous excitement with a secondary cerebral congestion; for, on the successive development of these states the treatment during the latter stages hinged. I wish you clearly to understand, that, after the headache and cerebral excitement which accompanied the very commencement of the fever had been subdued, or had ceased, after sleep and calm had returned, and had continued for many days, then a new order of things commenced—subsultus, watchfulness, muttering, raving, involuntary discharges, &c., all denoting great derangement of the nervous system; but still there was no proof that this derangement depended on cerebral congestion.

After a few, or after many days, however, unequivocal symptoms of the latter set in; the face and eyes became suffused and flushed; the pupils manifested a tendency to become contracted, and occasionally convulsions took place; the patient became also totally sleepless. When the latter and dangerous period of the fever was accompanied by the former nervous group of symptoms *alone*, they yielded to wine, musk, porter, and opiates; but when the symptoms indicating cerebral congestion were superadded, then it was that the case assumed so great and striking a similarity, so far as the functions of the nervous system were concerned, to the well-known variety of delirium tremens, accompanied by cerebral congestion, to which I before referred—to that variety of delirium tremens, in fact, which only can be successfully treated by the judicious, but bold exhibition of tartar emetic combined with laudanum. *It is the discovery of the utility of this practice in the advanced stages of spotted fevers, that I claim peculiarly as my own;* for there is not in the writings of any author on the subject the slightest trace of such a method of treatment to be found. As this method has manifestly saved many, many lives, under a combination of circumstances apparently hopeless, I cannot avoid congratulating myself upon being the first to propose a practice which has not only diminished the rate of our hospital mortality* in a remarkable manner, but has been the means of saving many of my friends and pupils; for without its adoption our class at the Meath Hospital would have been more than decimated, whereas at present we have to regret the loss of but one pupil.

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* Seventy-three fever patients—namely, forty-one males, and thirty-two females—were treated in the clinical wards at Sir P. Dun's Hospital during the months of February, March, and April. Of these, more than fifty were cases of maculated or spotted fever, and yet we lost but two females and one male. The latter was in a hopeless condition when brought in, and one of the former was attacked by varioloid just after the crisis of long-continued spotted fever.

One word more as to the circumstances under which this plan was applicable. They were exactly the circumstances which formerly would have been believed to demand the fresh application of leeches to the head, of cold lotions, and of blisters; for it was formerly argued, and justly, we have in this advanced stage of fever not merely debility to combat—not merely general nervous excitement to overcome—but we have also to contend with cerebral congestion. The latter is the most formidable of the whole; it was therefore said, let us meet it boldly; let us leech, let us purge, &c.; but I need not repeat to you the details of cases illustrating the ill effects of this practice. Suffice it to remark, that you might as well attempt to cure delirium tremens with mere leeching, purging, and blistering. Observe, I am now speaking of the advanced stages of fever; for where cerebral congestion takes place in the beginning or the middle of fever, then there is no room for opium—then will the practitioner have recourse to the well-known remedies for active cerebral congestion; viz., purging, leeches, cold lotions, ice to the head, &c. In the preceding sketch of the present epidemic, many important features have been omitted. The outline is only complete in such parts as were required to be filled up for the purpose of illustrating the principles which directed me in devising and employing this new plan of treatment. I cannot better illustrate these principles and their results for you than by the details of some additional cases; and first let me call your attention to that of Mr. Thomas O'Flaherty.

This young gentleman was seized with the usual symptoms of maculated fever, of an insidious character, and not attended with any appearance of danger during the commencement of the disease. His pulse never rose above 100, and before the seventeenth day of the fever it had fallen to 70, *at which it remained during the period of greatest danger*. The only circumstance which excited alarm in my mind, at an early period of his illness, was a great degree of mental apprehension, manifested in his anticipating an unfavourable result, together with a tendency to sleeplessness from the beginning. On the tenth, abdominal tympanitis was observed, but this was removed in two days by appropriate remedies. On the twelfth day he was very restless, and although he was perfectly rational in his answers to questions, and did not complain of headache, had neither flushing of face, nor heat of the integuments of the head, yet he frequently talked incoherently when left alone, and towards the latter part of the day began to make repeated attempts to get out of bed. On one occasion he succeeded, and walked down stairs, from his bedroom to the parlour. His tongue was brown and dry. Under these circumstances, I ordered him the mixture containing four grains of tartar emetic and one drachm of laudanum, in eight ounces of camphor mixture; of this he took two drachms every second hour. The effects produced by this medicine were not very rapid, but still they were decidedly beneficial, for he gradually became calmer, wandered less, did not attempt to get out of bed, and, during the night, got some sleep. His bowels being confined, the mixture was now laid aside, and purgatives exhibited; I should have remarked that the tartar emetic mixture caused profuse sweating.

On the fifteenth day of the fever, his bowels having been acted on, he was ordered twenty drops of Battley's solution of opium at night, which produced a comfortable night's rest—the first he had enjoyed since his illness. On the sixteenth, the sweating continued, the belly was fallen, and he was quite rational, but had marked subsultus; he got another dose of Battley, but it produced no sleep; he had been allowed chicken broth, beer, &c., for some days. On the seventeenth day the sweating had ceased, and his skin had become hot and dry; great restlessness, constant muttering delirium, subsultus, tremors, picking the bed-clothes, involuntary discharges: porter in small quantities, chicken broth, fœtid injection, and twenty drops of Battley at night. On the eighteenth, he was reported to have had no stool from the injection, and no sleep whatsoever. He answered incoherently, thought his bed was covered with lancets, some of which he collected carefully, and reserved for me; belly not tumid, but obstinately confined; pulse 100. The whole of that day, and the following, were employed in procuring alvine evacuations, preparatory to again giving opium; in the meantime, all his symptoms were aggravated, and when I visited him on the evening of the nineteenth day, his state was anxious in the extreme, as he had enjoyed no sleep for many days and nights, and was in a melancholy state of mental incoherence, raving, tremor, and subsultus.

Here came the crisis as to treatment. I remember well the time when a patient so situated would have been again purged, his head shaved, a few leeches applied to the temples, and a blister to the nape of the neck, while perhaps wine and musk would have been exhibited internally. How many persons have I seen so treated by the most eminent physicians, and how unsuccessful was the practice! To have talked of giving opium under such circumstances, and when the marks of cerebral congestion were so evident, would have been regarded as absurd; my experience on former occasions, however, determined me to give opium, and as the danger was imminent, I gave it boldly. To the eight ounce mixture, with four grains of tartar emetic, we added one drachm and a half of laudanum; of this he took one ounce every second hour, from eight in the evening until he had taken five doses. This produced copious sweating; the skin became cooler, he raved less, but still no sleep; at four on the following morning his pulse became 70, and respiration tranquil; he got twenty drops of Battley, and at half-past five in the morning, twenty-five drops more. He had now taken, within a short time, about one drachm of laudanum, and forty-five drops of Battley, combined with nearly three grains of tartar emetic. He was tranquil, but did not close his eyes, and muttered occasionally; subsultus less. His pupils now became more and more contracted, his eyes less expressive and duller, and when I came at eight in the morning, he was evidently deeply narcotized, although not yet asleep. I thought that all was lost; but still, observing the respiration to be tranquil, and the pulse regular, I indulged a faint hope that sleep might still supervene. His eyes now became still more inexpressive, the lids gradually closed, his breathing became prolonged and deep, and at half-past eight he was buried in a profound and tranquil sleep, which continued for nine hours, when he awoke, spoke rationally, said he had no pain in his head, took some drink, and fell asleep again. Next morning not a single symptom of fever remained.

The following cases prove that tartar emetic in considerable doses may be administered with advantage at a period of fever in which it was usually thought to be inapplicable, and to an extent which even now I cannot but consider as remarkable. When I first used tartar emetic and opium, I had not pushed the former remedy with the boldness and decision I have since done, for my experience only gradually accustomed me to a method of proceeding contrary to preconceived opinions, and my views of the powers of the remedy only gradually enlarged as I became more confident of its safety. It is but right to add, and I do it with gratitude, that I received much assistance and encouragement from the views of Dr. Marryatt of Bristol, published in 1788, but of which I and the profession in Ireland, and I may add in England, were generally ignorant until they were noticed in the first volume of the *British and Foreign Medical Review*, page 416. This notice of a work of which I had never before heard, and the testimony it contained that tartar emetic may be exhibited in considerable doses, and with advantage, at advanced stages of malignant fever, led me to attach more importance to this remedy alone, and uncombined with opium, and determined me to adopt a bolder line of practice in future—a determination which the event fully justified.

Some there are who will take occasion to remark that I can have no claim to originality on this occasion. But all who have watched my practice in the hospital, nay all who have taken the trouble of reading my lectures and successive publications on this subject, will at once acknowledge that I proceeded on this path of investigation with no other guide but an analogy derived from an observation of the effects of tartar emetic and opium in delirium tremens, a disease undescribed in the time of Marryatt. Every one the least conversant with the treatment of fever in private and in hospital practice in Dublin, London, and Edinburgh, will allow that no one, during the present century, ever taught or practised the exhibition of tartar emetic at the stage of typhus fever in which I have recommended it. Not a single hint at such a treatment is given in any of the numerous contributions on the treatment of typhus which form the valuable work edited by Dr. Barker and Dr. Cheyne. Where is there even one allusion to this practice in Armstrong, Smith, Tweedie? And what is said of it in Good, Thomas, Mackintosh, or in the *Cyclopædia of Practical Medicine?* Where is it mentioned or inculcated in the *Edinburgh Medical and Surgical Journal*, or in

Johnson's Medico-Chirurgical Review? Nowhere, although the treatment of fever is often the subject of anxious discussion.

So far suffices with regard to the novelty of the matter, for it is useless to argue with persons so stupid as to confound the practice I recommend with the well known and popular use of tartar emetic as an emetic or a diaphoretic in the commencement of febrile diseases generally. That I did not come upon this method sooner I regret infinitely, for since its adoption my practice in hospital and in private has been much more successful than formerly. Nay, shortly before Mr. Cookson's illness, I lost several of my friends, relatives, and patients, who would, in all probability, have recovered if so treated; and, among the rest, a gentleman the very week before the first trial I made of the practice in Mr. Cookson's case. I mention this fact as the strongest and most convincing proof that I had never even thought of this method until Mr. Cookson's case occurred, for, had I done so, I would have surely been inexcusable in allowing my patients to perish without even trying its effects. But it is time to proceed to the cases themselves.

A case occurred very lately in the Meath Hospital, where its progress was anxiously watched by many students and several practitioners, all of whom concurred in the opinion that the patient must have died had he been treated according to the plan usually followed under similar circumstances. This patient was attended, under my directions, by Mr. Harnett, who took the following notes of its progress, and visited the patient with unremitting attention both by day and by night.

Joseph Taylor, aged twenty-one, a strong young man of temperate habits, admitted into hospital on the 7th May, 1836. Ill seven days; sickness commenced with rigors, headache, pains in loins, &c. On admission he complained of headache, tinnitus aurium; face was flushed; eyes slightly suffused; was constantly frowning; skin hot and dry, slightly maculated; abdomen full and soft; bowels confined.

Habeat Haustum Rhei.

9th.—Slept pretty well; raved little; ringing in ears continues; headache increased; eruption of the maculæ much more copious; slight cough; some bronchitic rales over both lungs; abdomen in every respect natural; bowels regular;

pulse 100, distinctly dicrotous and sharp; tongue brown, dry, rough, and furred; had slight epistaxis three days ago.

℞. Pilulæ Hydrargyri, gr. iij.
Pulveris Ipecacuanhæ, gr. ss.; Misce, fiat pilula, 4tis horis sumenda.
Applicentur hirudines duæ naribus, et repetatur applicatio hirudinum vespere, si opus sit.

Tenth day of fever. Slept tolerably well; bled copiously from nares; pain in head diminished; countenance still flushed and hot; temperature of rest of body lower than natural; feet very cold; pulse 112, dicrotous and wiry; tongue parched and furred, dark brown; great difficulty in protruding it.

Stupes to feet, blisters to præcordial region, blisters to calves of legs in the course of the day.

℞. Misturæ Camphoræ, ℥v.
Liquoris Hoffmanni, ʒj.
Misce, fiat haustus, 4tis horis sumendus.

11th.—Became very violent yesterday evening; attempted to get out of bed frequently, but, when spoken to by the nurse, he remained quiet for a short time; was constantly raving and gnashing his teeth during the night; had no sleep; a short time before visit this morning, had a fit of an epileptic character, which lasted about ten minutes, in which he worked violently and foamed at the mouth. At the hour of visit, nine in the morning, the countenance was flushed, anxious, and expressive of great ferocity; eyes wild and suffused; pupils natural; complains of dimness of vision; eyebrows contracted; breathing hurried; is constantly tossing himself from one side of the bed to the other, and tearing the dressings off the blistered surface; skin hot and dry; abdomen soft; no tympanitis; bowels loose; tongue parched and furred; he is incessantly protruding and biting it, and gnashing his teeth; pulse dicrotous, very quick, and sometimes hard, but small.

℞. Antimonii Tartarizati, gr. vj.
Aquæ, f℥x.
Mucilaginis.
Syrupi Papaveris albi, āā. f℥j.; Misce, fiat mistura, sumat f℥ss. omni semihorâ.

Three o'clock, p.m. Has taken half the mixture, was nauseated by the second dose, but not since; he still continues very violent; fancies he has a bone in his mouth, which he is constantly biting; is in a copious perspiration since he commenced taking the medicine.

Mr. Harnett ordered ℥j. of the mixture every half hour.

Six o'clock, a.m. Appears a little calmer; has taken the whole of the medicine, no nausea produced; has bitten his tongue and lip severely; perspiration continues; has passed a large quantity of urine in bed; pulse soft and full.

℞. Antimonii Tartarizati, gr. iij.
Aquæ, f℥vss.
Syrupi simplicis, f℥ss.; Misce, fiat mistura, cujus sumat f℥ss. omni semihorâ.

Eleven o'clock, p.m. Has taken all his medicine without being nauseated; countenance less flushed; is constantly raving; pulse 100, full and soft.

℞. Antimonii Tartarizati, gr. iv.
Misturæ Camphoræ, f℥viij.
Tincturæ Opii, fʒj.; Misce, fiat mistura, cujus capiat f℥ss. omni semihorâ.

12th.—Continued raving during the night; had no sleep; appears much quieter this morning; face less flushed; eyes still wild and staring, but very slightly suffused; brows contracted; pupils natural; speaks rationally; pulse 80 and regular, has lost the dicrotous tone which it had yesterday; bowels confined.

Habeat enema emolliens, et repetatur mistura;
To have one pint of porter and chicken broth.

Three o'clock, p.m. Having taken the whole of the mixture, containing tartar emetic and opium, the simple tartar emetic mixture was again prescribed; after taking two doses of which he fell into a tranquil sleep, in which he is at present.

Eight o'clock, p.m. Has slept continually all day; awakes occasionally, but falls into a deep sleep very soon again.

Omittatur tinctura opii.

13th.—Slept soundly during the night; appears calm and must be pronounced to be large or small, not according to their weight or measure, but according to their effects, and when confessedly moderate doses are frequently given, and the effects of each carefully watched, surely caution herself can require no more. The same remark applies to my directions concerning opium.

The next case I have peculiar satisfaction in laying before you, inasmuch as its progress and treatment were witnessed by Sir P. Crampton, who was struck by the benefit resulting from a mode of practice he had never before seen applied, and that under circumstances which he considered as indicative of the greatest danger. Dr. Campbell, too, had an opportunity of witnessing for the first time this mode of treatment, and he since assured his class, that when I recommended it, he had scarcely a hope that our patient's life could be saved.

Mr. C., residing in Fitzwilliam Square, a surgeon, formerly an apprentice of the Surgeon-General, a young man of a powerfully athletic make, was attacked with the rigor of fever on Monday, 9th May, 1836. He was attended from the commencement by Dr. Campbell, and had a copious eruption of measles-like maculæ on the sixth day of the fever, when I first saw him. No unusual symptom occurred on the seventh day, and the headache, of which he complained much at the commencement, had disappeared in consequence of the application of a few leeches. On the morning of the eighth day we observed that every now and then he respired irregularly, as if repeatedly and gently sighing, a variety of respiration often indicating a disturbance of the nervous system, and which I have repeatedly observed as a precursor of cerebral excitement, and to which, consequently, I have been in the habit of drawing your attention, under the name of cerebral respiration.

On the afternoon of the eighth day we had the benefit of Sir P. Crampton's advice, who thought his case a very bad one indeed, for his pulse was almost 140 in a minute, and remarkably shabby, while he lay on his back thickly covered with maculæ; and we found that a rapid tumefaction of the abdomen had commenced within a few hours—a very bad symptom, inasmuch as the belly had been in the morning quite soft and fallen, and there was no cause to account for the sudden development of tympanitis, unless we supposed it, as it too frequently is, a harbinger of dissolution at no very distant period.

His tongue was parched, and he complained of thirst. The usual treatment by means of chloride of soda was determined on, in consultation; after which Sir P. Crampton expressed to the gentleman's friends the fears he entertained for the result. Scarcely had he gone out of the house, and just as Dr. Campbell and I were preparing to leave it, when a sudden change took place in our patient, who jumped out of bed, and nearly succeeded in throwing himself out of a garret-window. We found him violently delirious; but this state did not last for more than a few minutes, when it subsided into a delirium of a comparatively gentler description. He refused, however, to return to bed, and we were obliged to allow him to walk about in his shirt, supported, for he was feeble, by two attendants; his eyes became at times very prominent and ferocious; now and then he threatened all those about him, in a loud and terrifying tone of voice, and he seemed every moment on the border of frantic madness. Nothing could induce him to go to bed or allow even a blanket to be thrown over his cold and naked extremities. Thus seated on his chair, he presented a frightful picture, while his pulse became so quick that it could scarcely be counted, and was at the same time exceedingly weak.

What was to be done? The state of his circulation did not admit our endeavouring to control the cerebral excitement by arteriotomy or even leeches, and the last remark Sir P. Crampton made, was, that a very few leeches would kill him; blisters would be too slow in their action, and might even aggravate the disease; cold effusion seemed inadmissible. In short, it seemed that our patient was beyond the reach of all resources; as to tartar emetic, I felt at first unwilling to order it on my own responsibility, in a case apparently so desperate, and after Sir P. Crampton had left the house; in fact neither Dr. Campbell nor I thought it probable that our patient would survive twelve hours: yet, as I saw no possible means of saving him but the tartar emetic treatment, and determined at all risks to make a strenuous effort, I did not think myself justified in any longer hesitating about the matter, and ordered a mixture containing one ounce of syrup of white poppies, one of mucilage, and six of water, with eight grains of tartar emetic. Of this solution he was to get half an ounce every half hour, until a manifest impression on the cerebral excitement was produced.

The medicine was administered by the late Mr. Ferguson of Kildare Street, who told me afterwards that he was quite surprised at the treatment adopted, and was sure that neither it nor any other could save Mr. C.'s life. The first six doses seemed to sicken him a little, but he did not vomit until after the seventh dose; the eighth also produced very copious vomiting of mucus and bilious fluid. After the second vomiting he was prevailed on to go to bed, and was evidently more tranquil, but from having remained up uncovered for so many hours, much trouble was necessary before warm applications succeeded in restoring the natural temperature of his limbs and skin generally.

At 10 p.m. we saw him again, and finding that the medicine had produced so powerful an effect, we ordered it to be repeated only every second hour.

May 18th.—Ninth day of fever: 8 a.m. Has taken five doses since last visit; stomach quiet since the eighth dose. He slept several hours quietly in the beginning of the night (he had not slept for several nights before), but seems more excited now; he threatens some of his attendants, and appears likely to be unruly. It was therefore judged right to repeat the medicine every hour and a half.

1 p.m. Has taken eight grains of tartar emetic since six o'clock yesterday evening. A solution of the same strength in plain water was now directed to be given in the dose of half an ounce every fourth hour. He slept a good deal during the day, and the medicine operated on the bowels, bringing down very large fluid stools, consisting of a great quantity of healthy yellow fæcal matter. This effect is often produced by the tartar emetic in the advanced stages of fever, and is always a good sign. Although he was evidently more tranquil than before, it was thought advisable still to keep two strong, steady men constantly in the room, ready to assist the nurse in case of emergency. He still raved occasionally, and would not allow certain persons, me among the rest, to approach him, having conceived a strong aversion for us.

At 7 p.m. we found that the fever was again rising, and that the cerebral excitement was on the increase; we therefore again had recourse to half-hour doses, until the excitement yielded; after which it was given only every second hour.

May 19th.—Tenth day of fever: 10 a.m. He took six doses during the night. He got out of bed and eluded the vigilance of his attendants at a very early hour in the morning, but walked peaceably about the house, and when asked, returned quietly to bed. He slept well afterwards. As so much had been gained, we thought it unnecessary to persevere in the use of the tartar emetic; it was discontinued. He took in all twelve grains; it diminished the frequency of the pulse notably; and, what was very striking during the forty-eight hours we employed it, the pulse not only became slower, but much softer and much fuller; the skin became softer and moist; the belly was fallen and soft; and the maculæ much diminished. His fever, notwithstanding, still continued; he spoke incoherently, but did not again get out of bed.

On the fourteenth day an evident abatement of general fever commenced; the pulse fell, and the respiration, which, when he was at the worst, had been about 50 in a minute, fell to 25. This improvement continued progressive, and on the seventh day precisely, all fever left him; his pulse being then 60.

The after treatment consisted merely in giving a mild aperient every second day, until convalescence commenced. After the use of the tartar emetic had cured the cerebral excitement, he slept almost continually until the termination of the fever.

The next case is that of Mr. M., a gentleman of sedentary habits, full and corpulent, 40 years of age, who was lately attacked with violent symptoms of fever. He was very actively and judiciously treated by Dr. Ireland from the commencement. The measles-like eruption appeared about the fifth day. He had been copiously bled from the arm twice, and leeches were repeatedly applied to the forehead for the purpose of relieving pain in the head. He was likewise very freely purged. About the time the eruption appeared, his restlessness and debility increased, and he scarcely slept at night. In the course of a few days his state had become very alarming, and I saw him, in consultation with Dr. Ireland, on the ninth day of his fever.

We found that he had raved constantly during the preceding night, and was bathed in an exhausting perspiration, while the pulse rose to about 130; his perspiration was very frequent, and his face wore an evident expression of excitement, not of a violent but of a very restless character. His tongue was parched, and his body thickly covered with maculæ. In short, notwithstanding the active measures of depletion, general and local, applied in the beginning of the disease, it was evident that cerebral excitement had come on, and that too at a period of fever when debility forms a considerable obstacle to the further use of direct evacuants. His exceedingly gross habit of body, and prominent abdomen, were concomitants of the worst omen, for it is well known that very fat people seldom recover from typhus of a bad character. In this state of things tartar emetic was given to about the extent of three grains in the twenty-four hours; it was continued forty-eight hours, or until a satisfactory calm of the nervous system had been produced. Besides diminishing the delirium and inducing sleep, the remedy here brought away numerous and copious bilious stools, and diminished notably the frequency of the pulse and of the respiration. It is worthy of remark also, that, in proportion as he came under the influence of the tartar emetic, the useless and profuse perspiration began to abate, and after some hours ceased.

This gentleman's life was evidently saved by the treatment, for though his fever continued many days after, yet he never was in danger except from hiccup, which came on about the thirteenth day, and tormented him day and night. Claret, iced, seemed to have more power in relieving this symptom than any other expedient resorted to. His fever terminated about the nineteenth day.

Dr. Ireland, who has had the most extensive experience in fever, testified the pleasure he felt at witnessing the good effects of a mode of cure to him quite new, and applied in a case he thought almost desperate.

The following case presents so striking and convincing an illustration of the efficacy of my treatment, that I have thought it right to lay it before you also. The progress of the case was witnessed by several practitioners, who all declared, and I myself concurred in this opinion, that nothing would save the patient's life. His recovery was, without exaggeration, a matter of astonishment to us all; while at the same time it was so evidently the effect of the remedies employed, that many who had been wavering in their minds as to the utility of tartar emetic exhibited in the advanced stages of spotted fever, could no longer refuse their assent, and unhesitatingly declared their conviction that by no other plan of treatment could a favourable issue have been brought about. The patient was most diligently watched by Mr. Rooney, an attentive pupil, who visited him many times during the day and night, and reported to me the effect of the medicines.

Edward Meylagh, a stout, muscular peasant, aged 25, was attacked about the 23rd May, 1836, with the usual symptoms of commencing typhus. He was admitted into the Meath Hospital on the 1st of June, after the usual hour of visiting the wards. It was ascertained that he had been repeatedly and violently purged since the commencement of his illness by pills and aperient mixtures. I saw him at 9 a.m. on the second of June; he had passed a most restless night, muttering incessantly, and becoming at times so unmanageable, that it was necessary to put on the strait waistcoat. Now he is obstinately silent, will not answer questions, or put out his tongue when desired. His countenance is at once morose and haggard, and at times assumes a suspicious, ferocious aspect; eyes glazed, and slightly suffused; general surface of skin rather dry and hot, but his extremities are cold and livid; pulse 132, small and compressed; respirations 42, irregular; abdomen neither swollen nor tender; he passes urine and feces in bed; his tongue is dry, and dark-brown in centre, moist and red towards the edges. The whole surface of his body is covered with maculæ. Immediate attention was paid to restore the warmth of the extremities, and I directed him to get every hour half an ounce of a mixture, consisting of eight ounces of water, four grains of tartar emetic, and two scruples of laudanum.

1 p.m. At mid-day he began to gnash his teeth, knit his brows, screw his lips, and spit at every person that approached his bed. The expression of the face was rendered worse by the rapid motions of the eyeballs and a frequent squinting. In fact he became so ungovernable that the restraint of a strait waistcoat was no longer sufficient, and his legs and thighs were tied down to the bed. His carotids pulsated violently, and he alternately laughed and screamed aloud. Pulse 132, still small and wiry. As no perceptible action had been produced by the medicine, it was ordered in double doses.

6 p.m. Countenance much improved; less morose; he continues, however, to speak unconnectedly, but jocularly; is in a copious warm perspiration: pulse 120, soft and compressible; respirations 36, regular. To continue the double doses.

9 p.m. Has been in a composed, tranquil sleep since half-past six o'clock; perspiration continues; has passed a large quantity of urine; extremities are now naturally warm and moist; the pulsation of the carotids has subsided. He has taken four grains and a half of tartar emetic since morning, and twenty-three drops of laudanum. The medicine was now directed not to be given at regular intervals as before, but according as the symptoms seem to require it; it had neither nauseated nor purged him.

3rd June.—He has slept tolerably during the night, and got three doses of the bottle. About five in the morning he became somewhat restless, when a double dose was immediately administered, after which he slept composedly until nine o'clock, the hour of visit. His tongue is red, dry, and parched, fissured towards the tip; his thirst is increased, and he drinks very freely of cold water; skin moist and warm; pulse 96, dicrotous; respirations 30, regular; he seems inclined to sleep. His ideas are somewhat confused, although he answers rationally; bowels confined; abdomen a little tumid and slightly tympanitic. Has taken two grains and a half of tartar emetic and ten drops of laudanum since yesterday evening. I now thought it unnecessary to persevere any longer in the use of this mixture, and directed my attention to the state of the bowels, which soon yielded to emollient lavements. The alvine evacuations so procured were very copious, and were followed by immediate subsidence of the belly, and evident amelioration of the symptoms. He continued to sleep quietly during the day; at six in the evening his pulse was 90, soft and natural; respirations 30; skin warm and perspiring; maculæ have nearly disappeared.

7th June.—Much natural sleep; pulse 65, soft, of good strength, and without any of the dicrotous character; intellectual faculties rapidly improving; now passes urine and feces voluntarily; abdomen soft and fallen; tongue cleaning, and nearly moist. In fact, convalescence has almost commenced.

With one case more I shall conclude. A gentleman about 20 years of age was attacked with measles of an irregular form. The eruption did not come out favourably; and notwithstanding he was treated from the beginning by the late Dr. O'Brien, so well known as an excellent writer on the subject of fever, his state became daily worse, and Dr. O'Brien pronounced his case hopeless when he sent for me on the sixth day. It must be borne in mind that Dr. O'Brien was physician to the Cork Street Fever Hospital for thirty years.

The combination of symptoms which caused him to form this unfavourable opinion, was an exceedingly rapid, shabby pulse, violent delirium, total sleeplessness, and an evident sinking of the vital powers, manifested by coldness of the skin, &c., &c. As he was young, and the disease recent, we ventured to draw a little blood from the arm, but he fainted before many ounces could be obtained; we leeched his forehead without any perceptible effect. On the morrow he was worse: I then proposed the exhibition of small doses of tartar emetic, in frequently repeated doses. He took two grains in the course of ten hours; was nauseated or vomited by almost every dose; became more tranquil; finally fell asleep; and in twenty-four hours was out of danger.

Dr. O'Brien expressed to me in the strongest terms his gratification and surprise at the striking and beneficial application of a medicine he had never before seen given in like circumstances.

Another case of spotted fever, to which I was called by Mr. M'Nalty of Britain Street, afforded an equally favourable result within this last week; as did also a very dangerous case of the same disease, which I treated along with Mr. Mulock.

I have thus fully brought forward the result of my experience on this subject, convinced that I have not deviated in the slightest degree from the strict and naked truth in any of the preceding details. I have not in a single instance related what was not witnessed by other medical men of judgment, well known to the profession. If my treatment be not useful, it has singularly deceived me in curing my patients. If it be not new, it is strange that so many others in Dublin, that the whole body of practitioners, should have been fully as ignorant of it as I was myself.

I need scarcely again observe, that the proportions of the two powerful medicines which compose this mixture must vary according to the circumstances of the disease, and the age of the patient. In young persons of tender age, the opium must be given in smaller quantities.

Before concluding I may mention that since this practice was first proposed, it has continued to afford me the greatest satisfaction, and that I have reason to believe that those who have employed it in this country, and at the other side of the channel, have had no reason to lose confidence in it.

In a paper on typhus fever by Dr. Kilgour, we find that the experience of Dr. Dyce, of the Aberdeen Infirmary, is strongly in favour of this practice. He says:—"For months together the pulmonic symptoms prevailed almost entirely, then came those marked by gastric and intestinal irritation, and less often, though still continuing for a length of time in succession, those with high cerebral action. The first set, as is too well known, were by far the most intractable and fatal; the last, though sufficiently alarming, and always requiring restraint, were more amenable to treatment than either of the others, if anticipated in their approach, or seen soon after their onset. By the way, the medicine I *solely* relied on in this latter class, you do not include among your list—*I mean tartar emetic. Given as described by Dr. Graves, I have found it eminently successful, and have the greatest confidence in it.*"—*Edinburgh Medical and Surgical Journal*, vol. lvi. p. 389.

And in the eleventh volume of the *Dublin Medical Journal*, you will find an interesting paper on "Certain Remedies in Typhus Fever," by Dr. Hudson of Navan. Speaking of the treatment by tartar emetic and opium, he says:—"It seems best adapted to that restless kind of delirium tremens, in which the patient cannot be restrained from attempting to leave his bed, and walk about the ward; when every muscle is tremulous, the eye is red from want of sleep, the tongue dry, and the patient presenting that kind of spurious excitement which might induce the attendant (injudiciously, no doubt) to order the local abstraction of blood, by leeching the temples, or opening the temporal artery. I could here give reports from my note-book of several cases thus treated, but that I consider it would be rendering tedious a paper already too long. In prescribing this medicine, I find it advisable to use great caution in two ways: 1st, Not to give it *after* it has produced sleep; 2nd, To follow it up by the prompt and frequent exhibition of wine, and such nourishment or cordials as the more or less advanced stage of the disease and debility of the patient may require, as it seems to me that there is increased risk of the patient sinking unless timely supported after sleep thus induced."

To conclude, I must observe that I by no means wish to recommend tartar emetic as a specific in fever. I only use it in the complication above described. In fever the physician must use an almost endless variety of treatment according to the circumstances of the individual case before him; and he only will be successful who watches narrowly the progress of the cases intrusted to his care, and applies the appropriate remedies at the proper moment. Bleeding, leeches, purgatives, mercurials, absorbents, acids, stimulants, tonics, blisters, chloride of soda, may each be necessary in the treatment of different cases at different stages of their progress, or in different types. In fine, the treatment of fever will be always difficult, always complex, but it ought to be successful.

LECTURE XIX.

THE ADMINISTRATION OF WINE IN FEVER.—SEQUELÆ OF FEVER.

I CANNOT conclude the remarks I have to make on different points connected with the treatment of fever, without directing your attention, in an especial manner, to the *phenomena of the heart's action as an index for the administration of wine.* In the fifteenth volume of the first series of the *Dublin Medical Journal* you will find a paper on this subject from the pen of my distinguished colleague Dr. Stokes. From numerous observations he concludes that certain phenomena, which I shall presently detail, indicate a *softened* state of the heart, and that as soon as these phenomena present themselves, we should resort to stimulation by wine, &c. Dr. Stokes is of opinion that the pulse is a fallacious guide in fever, and that our attention should always be directed to the impulse and sounds of the heart for guidance either for the administration or withholding of stimulants, and he then details the peculiar characters by which this weakened condition may be recognised. I shall now read from Dr. Stokes's paper the leading doctrines contained in it:—

"We may thus arrange the cardiac phenomena obtained in our typhus fever:—

"1. Impulse and sounds remaining unaltered; the action of the heart corresponding with that of the pulse.

"2. Vigorous impulse, with distinct and proportionate sounds, with absence of pulse for many days.

"3. Diminution of both sounds of the heart, with absence or great diminution of the impulse (fœtal character).

"4. Diminution of the first sound, with cessation or great feebleness of the impulse.

"5. Complete extinction of the first sound, the second remaining clear.

"6. Predominance of the first sound, the second being extremely feeble.

"In the great majority of cases, however, the following were the phenomena observed:—

"1. Diminished impulse.

"2. Diminished first sound, particularly of the left cavities.

"With respect to the impulse, we arrived at some unexpected results. In most cases, considered through the whole progress, the diminution and return of the first sound were accompanied with the diminution and return of the impulse. So far the phenomena were what we might expect. *But in some instances, at particular periods of the case, this accordance between the impulse and sound did not exist.* In one case, the sounds became distinct before the impulse returned. In another the impulse became distinct on the eleventh day, while the second sound greatly preponderated. In a third case, we found that on the eighth day the sounds were not in proportion to the impulse; and on the tenth the impulse continued, but the first sound was totally absent. On the next day no impulse could be felt, yet the first sound was feebly audible. In the fourth case, the impulse on the twelfth day was less perceptible than on the day previous, but the first sound had more strength."

Dr. Stokes adds—"It is difficult or impossible, in the present stage of the inquiry, to offer any satisfactory explanation of these apparent anomalies; but it seems certain that, under the influence of the typhoid condition, the heart may have sufficient force to give an impulse with little or no sound, on the one hand; and on the other, its contractions may be accompanied by a sound, although the impulse be absent. Whether we are to explain these facts by referring to particular states of innervation of the heart, or to organic alteration of the muscular fibres or their connecting cellular membrane, is still to be determined."

Farther on Dr. Stokes says—"That the cause of the want of impulse, and feebleness or cessation of the first sound, is a *softening* of the heart, I have no doubt. The evidence in favour of this opinion may be thus stated:—

"1. That softening of the heart exists in typhus fever, as a local disease, and without any analogous condition of the muscles of voluntary life.

"2. That in our dissections in the last epidemic, we met with this softening of the heart in cases which during life had presented the phenomena in question.

"3. That the physical signs indicate a debility of the left ventricle principally, and it is this portion of the organ which is most often altered in consistence.

"4. Laennec has stated that, in proportion to the severity of the putrescent phenomena is the liability to softening of the heart; and the same observation is found to be true of the physical signs now described.

"The average period when these phenomena appear is about the sixth day, and they cease about the fourteenth day."

Dr. Stokes considers it highly probable that this softened state of the heart depends on an infiltration through its muscular structure of a peculiar secretion, identical with, or closely resembling that mentioned by Dr. Staberoh as occurring on the surface of the intestinal mucous membrane in cases of follicular ulceration.

"This, occurring in the heart, seems to impair its functions to a great degree; but the rapid restoration of the heart to health points out that the disease has not materially impaired its organic condition.

"Finally," says Dr. S., "I would draw the particular attention of my readers to the fact that, in the great majority of these cases, the use of wine was followed by the happiest effects. I may safely refer to the cases in proof of this proposition, *and I believe that in the diminished impulse, and in the feebleness or extinction of the first sound, we have a new, direct, and important indication for the use of wine in typhus fever.*"

I will now read the conclusions at which Dr. Stokes has arrived:—

"I. That the condition of the heart in typhus fever must be determined by the application of the hand and stethoscope, the pulse being an uncertain guide.

"II. That a diminished impulse, or a complete absence of impulse occurs in certain cases of typhus fever.

"III. That in such cases we may observe a diminished first sound, or even an absence of the first sound.

"IV. That both these characters may exist with a distinct pulse.

"V. That although in most cases the diminution of the impulse and first sound co-exists, yet that impulse may exist without corresponding first sound, and conversely, that the first sound may be heard although unaccompanied by impulse.

"VI. That these phenomena are most evident as connected with the left side of the heart.

"VII. That when the impulse or first sound are lessened or lost, the return to the healthy character is observed first over the right cavities.

"VIII. That in some cases both sounds are equally diminished.

"IX. That in a few cases the first sound preponderates.

"X. That these phenomena indicate a debilitated state of the heart.

"XI. That they may occur at an early period of the disease, and thus enable us accordingly to anticipate the symptoms of general debility.

"XII. That the existence of these phenomena, in a case of maculated adynamic fever, may be considered as pointing out a softened state of the heart.

"XIII. That this softening of the heart seems to be one of the local lesions of typhus.

"XIV. That the diminution or cessation of impulse, the proportionate diminution of both sounds, or the preponderance of the second sound, are direct and nearly certain indications for the use of wine in fever."

Though these doctrines are entirely new, and may appear to some rather fanciful, yet for their general accuracy I can vouch. I cannot agree, however, with Dr. Stokes, in attributing the phenomena of a *debilitated* heart to a *softening* of that organ, much less to the interstitial infiltration of a peculiar secretion analogous to that which Staberoh states he has observed on the mucous surface of the intestines in dothonenterite. On the contrary, I consider the heart, in typhus fever, to be affected with debility from the same cause which induces a debility of the voluntary muscles, and of the bladder and sphincter ani—that cause is a general prostration of nervous energy. That Dr. Stokes has seen the heart softened in the examination of subjects that had been affected with typhus fever, I have no doubt; but I would impute this condition to the effect of putrescence, a process which it is well known sets in with great rapidity in cases where death has been caused by any malignant disease. It seems difficult to conceive how the heart could contract in a case where "the right cavities were softer than natural, admitting the [illegible] through their walls without much resistance; and in which, in the muscular structure of the left cavities, this change was much more remarkable, the weight of the finger being almost sufficient to penetrate its walls, they were so exceedingly softened; it was very easily torn, and the edges thus separated had no longer the moistened appearance, but seemed as if quite dry. The septum cordis was equally softened; there was some dark fluid blood in the right cavities."

But the fact cannot be denied, that in many cases of typhus the heart becomes weak, that this weakness is manifested by a decrease in the strength of its impulse, or in the intensity of its sounds, or a change in their relative loudness and duration—and though I have never witnessed these changes without accompanying debility of the entire muscular system, and other evidences of prostration, yet I fully agree with Dr. Stokes, "*that in the diminished impulse, and in the feebleness or extinction of the first sound, we have a new, direct, and important indication for the use of wine in typhus fever*," and one from which the junior practitioner in particular will derive the greatest assistance.

But I also agree with Dr. Bell, the distinguished American editor of Dr. Stokes's Lectures, that, "important as is the guide thus furnished by the state of the heart for the use of stimulants, it may not be in the power of all, without some experience, to avail themselves of it. The practitioner will, therefore, do well to attend to the following points, as directed by Dr. Armstrong, in forming his opinion of the propriety of persevering in the administration of wine to a patient in typhus fever:—

"1. If the tongue become more dry and baked, it generally does more harm; if it become moist, it does good.

"2. If the pulse become quicker, it does harm; if it be rendered slower, it does good.

"3. If the skin become hot and parched, it does harm; if it become more comfortably moist, it does good.

"4. If the breathing become more hurried, it does harm; if it become more deep and slow, it does good.

"5. If the patient become more and more restless, it does harm; if he become more and more tranquil, it does good."

I have long endeavoured to impress on the minds of students the great importance of studying with attention that stage of fever in which wine and opium are occasionally the best remedies, with a view of learning what symptoms indicate their exhibition.

In the commencement of fever, we can decide with a good deal of certainty upon the most proper course of proceeding, but, as the disease advances, the symptoms become more complicated, the indications more confused, and the plan of treatment consequently doubtful. In this stage of fever it is that we must rely on the tact acquired by previous experience and reflection, and must often depend more upon a correct estimation of the general state of the patient, than upon the appearance or absence of any particular symptom. It is not my intention at present to do more than prove the truth of this assertion, by showing that the presence of some symptoms, commonly supposed to contra-indicate the exhibition of wine and opium, ought not to deter the practitioner from their use, provided that other circumstances seem urgently to require it.

1st. In the first place, as to the tongue, *at an advanced period* of fever I have often derived the greatest advantage from wine and opium, although the tongue was dry, the colour of old mahogany, or else coated with a yellowish brown fur, and protruded with difficulty, while the teeth and gums were covered with sordes. Wine and porter in moderate quantities seem *generally* to agree better with this tongue than opium; in some cases, however, the latter is indispensable.

For fear of misleading you, I must again remark, I by no means wish to assert that such a tongue uniformly, or even frequently, indicates the use of these medicines; on the contrary, this state of tongue and mouth will often be observed at a time when leeches and antiphlogistic treatment are required. Let it be clearly understood, however, that, at an advanced period of fever, this state of the tongue may exist, and yet wine and opium may be given boldly, provided, as I have said before, the general state of the patient seems to require it.

2ndly. The observations I have made concerning the tongue are applicable to *suffusion of the eyes*. The eyes may be heavy, a little red, very much suffused, and may have the singular expression of watchfulness, combined with great redness of the conjunctiva, which is termed a ferrety eye, and yet wine or opium may be the only remedy capable of saving the patient's life. It should always be borne in mind that the want of sleep tends to make the eye red, and that this condition is often, when it occurs in maculated typhus, analogous to the similar appearance of the eye which is observed both in measles and scarlatina, in which diseases it is merely a part of the general erythema, and does not contra-indicate the use of wine and opium if other circumstances call for their exhibition.

3rdly. A hot and dry skin does not necessarily contra-indicate the exhibition of wine and opium, particularly where there is at the same time a tendency to coldness of the extremities.

4thly. The presence or absence of delirium must always excite our attention when the question of giving wine or opium arises. I believe that these medicines are never applicable when the delirium is violent and continuous, but the patient may rave a great deal, particularly at night; he may mutter and speak to himself; he may point to various imaginary appearances, and may fancy himself surrounded by persons or things which have no real existence; he may be restless and irritable, constantly endeavouring to leave his bed for the purpose of walking about the room, or sitting at the fire; and yet he may be in a state urgently demanding wine and opium. On a more accurate examination, we find that his delusions are not so strong as to leave no room for the exercise of his reason. When spoken to emphatically, he answers in some cases incoherently, but in others with perfect precision and presence of mind, and does not for some minutes relapse into his former wanderings. This state of mind is usually accompanied by an almost total want of sleep, and, in many, by a great anxiety about their illness. To procure sleep, as has been well remarked by Latham, in a late number of the *Medical Gazette*, is here one great object, and this can only be done by means of wine and narcotics. In some the mental aberration is scarcely perceptible, and they have all the characters of great excitement of the nervous system, without any actual raving or delirium. There is general tremor and subsultus. The tongue is tremulous when protruded, or when moved in speaking, and consequently the articulation is uncertain and interrupted, while, in general manner and mode of answering questions, the patient strongly resembles a person affected with delirium tremens.* This group of symptoms is likewise accompanied by want of sleep, and best treated with wine and opium.

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* It is in these particular forms of fever that I have discovered the great utility of tartar emetic and opium; see last lecture.

5thly. The appearance of the face has been much relied on by some, as capable of guiding us in forming our decision. Heat of head and face, redness of the cheeks, and strong pulsation of the carotids, are well known as contra-indicating wine or opium; but in the advanced stages of fever, the face, like the eye, may be suffused, it may be seen occasionally flushed, and when flushed, it may be hot, and yet wine and opium may, nevertheless, be our only resource.

6thly. Headache, when violent, is at any period of fever a decisive circumstance. Sleep cannot be obtained while the pain is unmitigated, and we must, therefore, attempt to conquer it by the most active treatment, by local applications to the head, by depletion from the vascular system, and by purgatives. Sometimes, however, these means fail, and the physician feels that he cannot pursue this mode of treatment any further. Under such circumstances, a dose of opium boldly exhibited will occasionally succeed in procuring sleep, from which the patient awakes nearly free from headache. Before having recourse to this remedy, the effects of a blister to the nape of the neck ought to be tried. In the more advanced stages of fever, the headache, or rather the heaviness felt in the head, is something very different from the throbbing, acute headache just spoken of, and constitutes no contra-indication to the use of wine and opium.

7thly. The state of the pulse requires to be duly considered. Its frequency is not of much importance, for I have seen wine and opium prove highly serviceable in all its varieties, from 70 to 130, or even upwards. No one would ever think of exhibiting these remedies when the pulse is strong, and more particularly when it is strong and hard; but the case is otherwise when it possesses only a certain degree of *hardness*, and is at the same time small and thrilling, not resisting compression with the force the sensation of its hardness leads us to expect.

Such are the chief observations I have made on the particular circumstances and symptoms supposed capable of throwing light on this important practical question. They may serve to prevent the student from being misled by rules of practice dogmatically deduced from the observations of any single symptom, and may lead him to turn his attention more accurately to the previous progress of the fever, and the general state of the patient. It is almost superfluous to add that, when any doubts exist concerning the propriety of giving wine and opium in fever, they should not be tried unless their effects be carefully watched by the physician himself.

Permit me next to call your attention to some of the sequelæ of fever, and first to some points connected with sudden and violent delirium succeeding maculated typhus fever. It may be doubted whether any writer has illustrated with sufficient details the fact that delirium of a most violent and dangerous description sometimes suddenly supervenes in patients who, to all appearance, have passed favourably through the various stages of maculated fever. I published on a former occasion the case of a student in Trinity College, who was thus attacked on the eighteenth day, at a time when he seemed to have passed the crisis favourably, his pulse having fallen to 60, and all other symptoms of fever having disappeared; since that observation was made, I have seen so many cases of a similar description, that I think it right to impart whatever additional experience has taught me concerning the history and treatment of this singular species of delirium. It will appear evident, from the nature of the means successfully employed in treating this affection, that it has little or no affinity to the delirium which, in the first stages of fever, so often accompanies true inflammation or congestion of the brain, but is rather allied to delirium tremens, delirium traumaticum, and acute puerperal madness. As in each of these the delirium is preceded by the operation of some cause, which acts unfavourably on the nervous system; so in the delirium we are now about to consider, the pre-existence of fever may be assumed to act in a similar manner. Neither does maculated fever seem more inadequate to produce so serious an effect, than the act of parturition, the presence of a wound or fracture, or the long-continued abuse of intoxicating liquors; for no severe typhus fever ever runs its course without bearing heavily on the nervous system. The facts I am to relate ought to make physicians extremely cautious about pronouncing fever patients out of danger; for even after a crisis, occurring in due time, and apparently the most satisfactory and complete, delirium may suddenly arise, and may place the patient in the greatest peril, the physician having, perhaps, taken his leave, in the full assurance that his visits were no longer necessary.

Four years ago I attended, with the late Mr. King, a gentleman in Grafton Street, who had fever without any remarkable symptom, or anything that required the adoption of active measures. He had maculæ, it is true, but the patient was young, and went through the disease favourably; on the sixteenth day his pulse had fallen to sixty, and all danger seemed over. He had no thirst; his tongue was moist; eyes clear; and not the slightest headache, or appearance of cerebral determination: in fact, when I visited him on the morning of the seventh day, everything betokened a speedy recovery. I must observe, however, that in this, as well as in most cases of the kind I have witnessed, there was a certain degree of nervous excitement present, tending to produce want of sleep, and consequently on leaving him at my evening visit, I directed the nurse to give him an opiate draught. This was unfortunately omitted; the young gentleman became gradually more restless and agitated, began to rave, and was found by Mr. King, next morning, in a state of high delirium. His pulse was still rather slow, not mere than 60 in a minute; his skin was cold; his countenance collapsed; and he had been during the night wholly sleepless. We had great difficulty in managing this patient; and it was only by means of great attention, stuping his legs, a nutritious diet, wine, and black drop, exhibited freely and repeatedly, that his life was saved.

Another case of the same kind, and calculated to excite great interest, was that of a pupil of the Meath Hospital. This gentleman was attacked with the prevailing fever, and like most patients, exhibited maculæ about the fifth day. There was, however, nothing very remarkable in his disease, no symptoms of anomalous character, or of a severity requiring very active measures. When first attacked, he felt rather nervous; but this was very little to be wondered at in a person who had been studying intensely for a considerable time.

At a very early period he exhibited a tendency to tremors and subsultus tendinum; but all his other symptoms were mild, and by strict attention, and the kind care of his fellow-students, he went through the disease favourably, and appeared quite free from danger on the sixteenth day. On the seventeenth day I found him, at my morning visit, in a very promising condition, his pulse down to 60; his tongue moist; his skin of a natural temperature; and his eye clear, and nothing present but a certain degree of nervous excitement. To counteract this tendency the late Dr. M'Dowel and I had found it necessary to give him every night an enema containing twenty-five drops of tincture of opium. Unfortunately this was omitted for one or two nights about this period. The fever resolved itself; but resolved itself during the period of sleeplessness, and a certain degree of nervous excitement. I saw him on the morning of the eighteenth; I thought there was a good deal of anxiety and quickness of manner about him, with some slight increase in the muscular tremors. I therefore wrote to Dr. M'Dowel, and begged him to see that he took his opiate that night. Before this was done, he grew much worse; in the evening he became highly excited, then quite delirious, and towards morning it was necessary to call in the assistance of three or four persons to keep him in bed. Dr. M'Dowel continued to attend him with great care and skill, and had sufficient influence over him to make him swallow the requisite medicines, which no one else could. Opiates were at first tried, but failed; we then commenced with the free exhibition of tartar emetic, and extract of belladonna; in the course of twenty-four hours he took five or six grains of the latter; we afterwards omitted the tartar emetic, and substituted black drop in its place: this succeeded, and after a violent attack of delirium, which lasted for thirty-eight or forty hours, he fell into a deep sleep, from which he awoke refreshed and rational. It was necessary, however, to repeat the narcotics for several nights, and they were not omitted until his convalescence became so confirmed as to remove any apprehension of a relapse.

Here are two cases in which the disease declines, and the patient is regarded as nearly convalescent, when suddenly cerebral symptoms of a most alarming character manifest themselves. The fever subsides, but with nervous excitement and insomnia, circumstances which have been long observed as characteristic of an imperfect crisis. The point, however, to which I wish to direct attention is, that a person not thoroughly acquainted with the nature of this affection might be led into a very important error. He might, perhaps, suppose this to be inflammatory excitement, to be treated by leeches, cold to the head, and other antiphlogistic measures. In the first case, indeed, the symptoms were so violent that I advised leeching;

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but Dr. M'Dowel did not apply them, and perhaps it was well that he did not. I do not mean to say that leeches and the antiphlogistic treatment are never indicated in the delirium which occurs at an advanced period of maculated fever, or in that which follows the stage in which the pulse falls to the natural standard, and thirst ceases, and the skin grows cool. Such an assertion would lead, in some instances, to an injudicious and even dangerous method of treatment; for cases do occur where, under these circumstances, topical antiphlogistic measures are absolutely called for. My object in making these remarks is to point out, not the rule, but the exceptions, the numerous exceptions to the method of treatment usually employed. In the delirium I am now describing, the feet and legs must be constantly stuped, the head must be diligently sponged with warm water and vinegar, the bowels relieved by injections, while opium is exhibited by the mouth or in *lavements;* where there is warmth of the scalp, and the temporal arteries full, leeches are required, but where the scalp is not hotter than natural, they would prove hurtful; in a state of collapse, wine may be necessary; *blisters to the nape or head seem to increase the delirium.* When leeches are indicated, their good effects are much enhanced by combining tartar emetic with the opium, provided no diarrhœa or other symptom of abdominal irritation can be detected.

The next case is even more remarkable than the preceding, for the delirium came on quite suddenly and without any premonitory symptom, and did not commence for several days after the fever had entirely ceased, which it did about the seventeenth day; neither was the termination, in this instance, rendered suspicious by any previous want of sleep.

Mr. ——— was attended by Dr. Brereton, who found him labouring under the usual symptoms of fever, which commenced about the 27th of January, 1835. He was a young man of excellent constitution, and temperate, active habits: soon after the commencement of the disease, some bronchitic symptoms appeared, and at the usual time the maculated eruption was observed. Nothing remarkable occurred until towards the fourteenth day, when a notable and steady improvement commenced, and consequently I left off my attendance, having been called in about the seventh day of the disease. As the patient's constitution was sound, his friends were not likely to permit any error of diet, and I did not anticipate a relapse, especially as there had been no serious affection of the brain, chest, or bowels during the course of the fever. I left him cool, cheerful, and self-possessed, his pulse regular, about sixty in the minute, and head entirely free from pain or flushing; his tongue had become clean; thirst gone, and appetite returning. All these particulars were of the most encouraging description, and were not counterbalanced by any symptom indicative of the fast approaching danger. On the following day, the eighteenth from the beginning of his fever, I was again sent for in haste, and found that the patient had become suddenly and outrageously delirious during the night, an occurrence which seemed the more surprising, as no other symptom existed denoting a return of fever. This gentleman's life was saved with great difficulty, for the delirium continued several days, and was at last only appeased by considerable doses of tartar emetic, combined with musk and opium.

There is one fact connected with the history of fever which should never be forgotten by those who are occupied in its treatment: I allude here to the occurrence of sudden accidents, or the supervention of other diseases, producing a material alteration in the circumstances of the case, and leading to new and more alarming dangers. You should not divest yourselves of all further anxiety for the patient, or relax in your attentions, because the fever has exhibited a tendency to decline, and a favourable crisis has taken place: crisis may occur, and convalescence may be established, and yet the patient may relapse, or he may be struck down again by the unexpected incursion of a new and dangerous malady, or he may expire suddenly in the course of a few minutes. The functions of the brain and heart may suddenly give way, and death may take place unexpectedly and at once. Thus, it not unfrequently happens that a patient during his convalescence falls into a state of syncope, from remaining too long in the erect posture, and if assistance be not promptly afforded, life is speedily extinguished. In the state of debility which follows acute and exhausting diseases, and where the patient is very liable to syncope, the most assiduous attention is required. During the epidemic of 1826, death took place under such circumstances in five or six instances, and the convalescents lost their lives from incautiously sitting up or walking about the room too long, or attempting to reach the night-chair without assistance. There are many other causes capable of producing a sudden and alarming change in the state of convalescents from fever. One of the most obvious of these is error or excess in diet, which is apt to bring on a return of the fever in an aggravated form, accompanied by symptoms of gastro-enteric inflammation, and sometimes terminating fatally in forty-eight hours.

I shall now proceed to lay before you a sketch of a very important form of disease which attacks convalescents from fever, and runs a course of remarkable intensity and rapidity. I am not aware that this form of disease has been described by pathological writers: the nearest approach to a description of it is an account of the swelled leg which occurs after fever, given by a Glasgow physician. Dr. Stokes and I have given a description of a swelled leg after fever, as observed during the epidemic of 1826, but the important and fatal form of the disease which I am about to describe, did not come under my notice until within a more recent period.

Before the commencement of the present session, a fine young woman, aged twenty-four, previously healthy and robust, was admitted into our fever ward. She was admitted on the 26th of September, having been at that time eight days ill, and labouring chiefly under gastric and cerebral symptoms. Her treatment consisted in the application of leeches to the epigastrium and head, cooling drinks, and blue pill combined with James's powder. Under the use of these and other appropriate remedies, the fever declined, and on the 1st of October the cerebral and gastric symptoms had disappeared, and the patient complained merely of a slight degree of feverishness. On the 2nd of October she was seized with rigors and horripilation, followed by intense pain of the left mamma, accompanied by numbness and loss of power of the corresponding arm. She was leeched with some relief, but passed a sleepless night, and next day an oblong patch of redness was seen extending upwards from the nipple; the pain was still violent, and she could not bear the slightest touch on the affected parts. The breast was leeched again, and fomented assiduously during the day. On the fourth the erysipelas was spreading, and the pain was still agonizing. She screamed out whenever it was touched, and could not bear even the weight of her dress or covering. On examining the breast, no enlargement or hardness could be observed; there was no remarkable heat or tension, and with the exception of a slight erysipelatous redness, and pain rivalling that of tic douloureux in severity, there was nothing to indicate the presence of the disease. The left arm continued numb and powerless.

This state of things was accompanied by remarkable increase of fever, as manifested by foul tongue, accelerated pulse, and sleepless nights. She now began to complain of dull pain in the calf of the right leg, aggravated by pressure or motion, but not attended with any apparent increase of heat, swelling, or induration. On the fifth she is reported to have passed a sleepless night, although the watery extract of opium had been administered freely on the preceding day and evening; the erysipelatous redness had extended nearly as high as the clavicle, and the affected parts had now begun to swell considerably. On the sixth she is stated to have had some sleep, and the erysipelas was extending, in some parts covered with vesicles. She again complained of cramps in the right leg, and on making an examination we found considerable tenderness on making deep pressure, but no external indication of disease. Her debility was increasing, accompanied by a tendency to looseness of bowels, for which she was ordered enemata of sulphate of quina and laudanum. On the following night she was attacked with intense pain in the leg, accompanied by exquisite tenderness to the touch, but no redness, swelling, or increase of temperature. The erysipelatous affection of the breast had now become pale, and ceased to spread. The enemata were continued, the parts dressed with mercurial ointment and extract of belladonna, and wine freely allowed.

She passed the night in great agony from the intense pain in the leg, and complained of frequently recurring rigors followed by perspiration. She also stated that for the last two or three days she had experienced repeated attacks of tremor in the affected limb; one of these tremors attacked the limb on the night of the eighth, and continued for three or four hours, terminating in copious general perspiration. These increased on the following day, attended with increase of fever, thirst, and debility, and the pain in the leg continued with unabated violence. It is worthy of remark that at this time there was no erysipelatous redness or discolouration of the affected limb, and scarcely any swelling. On the ninth she is reported to have passed the night screaming and sleepless, she vomited three or four times, complained of intense pain in the abdomen, and had a violent rigor which continued from one o'clock to six in the morning, followed by profuse perspiration. The right leg continued exquisitely painful as before, became somewhat swollen, and its veins appeared more prominent than natural, but there was no discolouration of the integuments. Both arms were now painful on motion, and the left leg became painful and tender on pressure. Under this complication she sank rapidly, and died at three o'clock in the afternoon.

On dissection, purulent matter was found under the integuments covering the left breast, but the gland itself appeared healthy. There was no vascularity nor other traces of peritoneal inflammation, and the abdominal viscera were healthy. The right leg was infiltrated; its veins were pervious and elastic, but their internal coat exhibited a rose-coloured tinge.

Here, then, we have a very remarkable and formidable train of symptoms, arising without any obvious cause, running a rapid and fatal course, and exhibiting a character of singular intractability. From all that we had previously seen or heard, this young woman's constitution was robust and healthy, her fever had been treated successfully, and she appeared to be getting over it without any sinister accident, or any complication capable of disturbing her convalescence; yet at this period she is attacked with fever of a new type, accompanied by local affections of the breast and extremities, which run a rapidly fatal course, and exhibit phenomena of a new and extraordinary character. She is first attacked with erysipelas of the left mamma, accompanied by pain and loss of power of the corresponding arm; then she gets exquisite pain of the right leg, and then of the left leg and right arm; in fact the whole four extremities are more or less implicated.

Now by what name should we designate this affection, or what would be the most appropriate term to apply to it? Was it phlebitis, or erysipelas, or phlegmasia dolens? The affection of the mamma certainly resembled erysipelas, but differed from it in the agonizing character of the pain, and I have already observed that in the legs or arms there was no appearance of redness or discolouration. That it was pure phlebitis I think we are not authorized in concluding, from the phenomena observed on dissection. There was no pus in the veins (an occurrence which might naturally be expected from the acute character of the disease), no thickening or induration, the coats of the veins were elastic, and to all appearance healthy, with the exception of a rose-coloured tinge. Now considering the previous state of the woman's system, I do not think that we can conclude as to the existence of pure phlebitis on such slight grounds, or say that the whole group of symptoms which characterized the secondary attack depended solely on inflammation of the veins.

The disease of which I speak simulated in many points phlegmasia dolens, but differed from it in the phenomena observed in the breast, as well as its more general effusion, and the absence of that peculiar whiteness of the affected limb which characterizes the latter affection. It appears to be a form of disease resulting from the generation of a morbid poison in the system, and manifesting itself in diffuse subcutaneous inflammation of a low and cachectic nature, affecting primarily the skin and subcutaneous areolar tissue, and afterwards involving all the subjacent parts more or less according to their different susceptibilities. It was accompanied from the commencement by increased irritability of the muscular and cutaneous nerves; indeed in the case just detailed, the nerves appear to be parts primarily affected. Another remarkable circumstance connected with this case is the loss of power observed in the affected limbs. In all cases where a severe and painful affection of the nerves is present, you have more or less loss of power, but as far as my observation has gone, there appears to be a difference in the derangement of muscular motion connected with painful affections of large nervous trunks, and that which accompanies an affection of the terminating fibrils or nervous extremities. In the latter case the degree of paralysis is always more considerable; of this, phlegmasia dolens affords a good illustration. In this disease the extremities of the nerves are chiefly affected, and the loss of power is always greater than when a large nervous trunk is affected, as for instance in sciatica. In the latter affection the pain is often extremely violent, but the motion of the limb is never so much impeded as it is when the nervous extremities are the parts chiefly engaged.

You perceive, then, that the affection which I have just described consists in the development of low, malignant, and irregular inflammatory affections in various parts of the body, but particularly in the extremities, commencing probably in the subcutaneous areolar tissue, but subsequently extending to all the neighbouring parts, and exhibiting many of the characters of those inflammations which result from the presence of an animal poison in the system. A peculiar feature of this affection, also, is the intense neuralgic pain which accompanied it, and I think it might with some propriety be designated as neuralgic diffuse inflammation after fever. It is accompanied by fever of a peculiar type, ushered in by rigors, and characterized by remarkable derangement of the digestive canal, debility, and sleeplessness. A point, also, which deserves notice in this case was the recurrent rigors and perspirations, marking the occurrence of new and additional mischief, and indicating the malignant and intractable nature of the disease.

One word as to the connexion of this disease with phlebitis. Some pathologists are of opinion that phlegmasia dolens and swelled leg after fever are nothing more than modifications of phlebitis. I cannot, I must confess, agree with this opinion, nor am I prepared to admit that the symptoms in the foregoing case were referable to mere inflammation of the veins. I do not deny that the veins may be affected, but phlebitis is not the first link in the morbid chain, and is itself merely a consequence of the same unknown cause which determined the inflammation of other tissues. I beg leave to observe here that the affection I have just described seems like others capable of existing in very different degrees. Thus, I have seen some instances in which there was no other symptom but severe pain of the extremities, generally about the calves of the legs, and which was relieved by warm fomentations and mild aperients. Again, I have seen, in addition to this symptom, swelling and tenderness of the legs, which, however, generally yielded to leeching and other appropriate means. Probably we are authorized from this and other facts in concluding that the disease is not always of a malignant and fatal character, and that there are at least certain forms of it amenable even to simple and ordinary treatment: you should, however, be always on your guard when patients recovering from fever are attacked with pain, in the lower extremities particularly, as this symptom not unfrequently ushers in a serious and alarming disease.

The next case of this disease observed in our wards occurred also in a young woman, named Dillon, aged 23, and apparently of good constitution. She was admitted into the fever ward on the 2nd of September, being at that time about seven or eight days ill. She had on admission the usual symptoms of fever, accompanied by intense bronchitis, dyspnœa, costiveness, and loss of sleep. Under the use of cupping, blisters, calomel, and other appropriate means, the fever and pulmonary symptoms declined, and she was pronounced convalescent on the 12th. On the 18th she had been up as usual, but towards evening complained of rigors, and said she felt her right leg very painful. The pain of the limb continued next day, intermitted during the following night, but returned on the morning of the 20th with increased violence. She was leeched without much relief, and on the 21st she is reported to be extremely feverish, her pulse frequent, her tongue foul, bowels loose. She had passed a bad night, and the leg was still exquisitely painful and somewhat swollen. She had twelve leeches again applied with some relief, but on the 22nd the left shoulder became similarly affected with pain, and so tender as not to admit of the slightest pressure. On the 23rd there was some diminution of pain in the leg and shoulder, but her pulse was jerking and unequal; her tongue parched; her countenance anxious; and she complained of intense pain in the small of the back. She passed a sleepless night, and next day complained of exquisite pain in the left lower extremity. This was accompanied by an exacerbation of the febrile symptoms; she moaned constantly; her pulse became excessively feeble and rapid; and she died on the 24th.

On dissection, the peritoneum, particularly that portion of it attached to the abdominal parietes, was found remarkably vascular, the vascularity being most intense over the hypogastric region. There was no effusion of lymph or serum, but about half an ounce of purulent fluid was discovered in the cavity of the pelvis. The viscera were healthy. The internal surface of the principal venous trunks was tinged red, and there was a small quantity of coagulated blood in their cavities. On making an incision into the right leg, along the course of the internal saphena, the subcutaneous areolar membrane was found infiltrated with sero-sanguineous fluid; the texture of the veins was here apparently natural, their cavity pervious and filled with fluid blood, without any lymphy or purulent admixture. No distention or enlargement of the lymphatics was observed.

Here you have a case corresponding in its main points with the former, and differing from it chiefly in being complicated with peritoneal inflammation and synovitis of the shoulder-joint. Its origin was similar; it exhibited the same kind of intense neuralgic pain; the same fever; the same extensive diffusion of local inflammation, and the same unfavourable termination. The chief points of difference were that in the latter case the disease attacked the synovial membrane of the shoulder-joint, and the serous membrane of the abdominal cavity. This, however, is by no means unusual. As to the synovitis, I have observed it in more than one instance after fever. I have witnessed a very remarkable instance of it in a man in this hospital who was attacked with swelled leg after fever. In addition to the affection of the leg, he had also synovitis of the knee-joint of so severe and intractable a character that he recovered with difficulty, with an anchylosed state of the joints. On the whole, the disease which we have been considering is one of great importance, and deserves particular attention. It is sometimes of a very unmanageable character, and baffles our best directed efforts. The treatment which appears best adapted for it consists in leeching, fomentations, and the application of mercurial ointment with extract of belladonna to the affected parts: these combined with the internal use of quina and opium, with occasional doses of calomel, seem to comprise the chief remedies on which we can place any reliance.

Before I conclude this lecture I shall allude briefly to the very interesting case of Sarah O'Neil. This young woman was admitted on the 17th of February, having been attacked on the 10th with fever of the ordinary type. On the day after her admission she complained of want of sleep, and pain of the forehead and temples; but she had no raving, tinnitus aurium, intolerance of light, or other symptom of inflammation of the brain. She had been confined about a fortnight before she came in, and complained that her breasts were very troublesome to her. Her belly was soft and fallen, quite free from tenderness or soreness, and she stated that her bowels were free. Her tongue was furred, her pulse 130, the lochia suppressed for the last two days. Things went on tolerably well for four or five days, when her belly became tympanitic, she began to complain of pain on pressure. The action of the heart now became more violent; her pulse rose to 140, and blood began to appear in her stools. On the 24th of February—that is to say about the fourteenth day of her illness—her pulse was 150; she passed a large quantity of blood from the bowels, and the tympanitis subsided.

In cases of fever accompanied by tympanitis and signs of intestinal congestion, hemorrhage from the bowels, particularly when it occurs on one of the critical days, should not be interfered with. It is in this way that nature very frequently brings about relief of the congestion and irritation of the gastro-intestinal mucous membrane, just as she relieves congestion of the head by bleeding from the nose. In the case of a lady whom I attended along with Mr. Palmer, some time ago, at Drumcondra, the occurrence of intestinal hemorrhage was followed by the most marked effects; her belly became soft, the tympanitis disappeared, and all her febrile symptoms were speedily removed. The appearance of blood, therefore, at such periods and under such circumstances, is to be looked on as a favourable occurrence; nor should it be interfered with in any way until, from its continuance or its quantity, it appears likely to produce debilitating effects.

In the present case, however, this hemorrhage will require to be very carefully watched. The woman's system is that which is favourable to profuse fluxes of blood, for it is not long since her accouchement, and she has suppression of the lochia. She has had but little fever for the last two or three days, but the action of the heart still continues extremely violent, and her pulse is still rising. Respiration, too, has been considerably accelerated, and, where this occurs, you have always reason to apprehend danger. I have accordingly endeavoured to moderate the hemorrhage by the use of acetate of lead and opium. A draught composed of acetate of lead, eight minims of tincture of opium, and fifteen minims of wine vinegar, in six drachms of water, has been prescribed to be taken as occasion requires. A large blister has been applied, so as to cover the epigastrium and sternum, and she has been allowed port wine and chicken broth. Where a patient, debilitated by previous fever, has been attacked with hemorrhage, you should be careful in supporting the system by small quantities of wine and light nutritious food; for there is always more or less danger to be apprehended of a sinking of the powers of life. In cases of this kind the cautious use of acetate of lead, with opium and wine, are the only means on which we can rely with any confidence.

LECTURE XX.

NERVOUS FEVER.—CAUTION AS TO PROGNOSIS.—PRESCRIPTIONS IN FEVER.—CONCLUDING REMARKS.

PERMIT me to make one or two observations on a case of which I have already spoken, and which, as I expected, has terminated fatally. A man named Lynam has been lying ill for a long time in a large fever ward; I wrote at the top of his card "Nervous Fever," and remarked to the class that his disease was pure fever, of a nervous type, unaccompanied by any symptoms indicating decided local inflammation. You will recollect that his symptoms were heat of skin, quick, weak, compressible pulse, thirst, watchfulness, and low muttering delirium, unattended with any appreciable sign of visceral disease, or any symptoms denoting a putrescent state of the fluids. It was not congestive or putrid, or gastro-enteric, or petechial fever; neither could it be called a cerebral fever; it was only by separating from it the idea of each of these species, and by studying its negative characters, that you could arrive at something like an accurate conception of the type of the disease. It was, as I have already stated, nervous fever, modified by the patient's previous habits of long-continued intemperance. When a patient addicted to intemperate habits gets an attack of fever from cold, fatigue, or exposure to contagion, you will generally find the disease exhibit a compound or mixed character, the phenomena of fever being combined with those of delirium tremens. And so it was in this case; the man had general tremors, with persistent watchfulness, and muttering delirium.

His treatment consisted in the employment of medicines calculated to soothe the nervous system, and I kept a constant watch over the state of the principal viscera. About a week after he came under my care, and about five weeks from the commencement of his fever (for he was nearly a month ill before he came to the hospital), he was attacked with erysipelatous inflammation of the face and scalp. The disease commenced on the face, and, travelling upwards, very rapidly attacked the whole scalp and back of the neck, its progress being accompanied by great aggravation of symptoms. At that time I remarked to the class that I did not entertain any apprehension of a metastasis of the erysipelas, that I had no fears of the supervention of inflammation of the brain, and its train of alarming consequences, but that no good was portended by this attack of cutaneous inflammation, and no relief of the internal parts could be expected from it, for every symptom appeared aggravated from the moment that the erysipelas commenced. I pointed out the total inadmissibility of anything like vigorous or antiphlogistic treatment, in a case where the disease had appeared in an individual of broken constitution, labouring under a combination of delirium tremens with low fever; and said that even the remedy which we had found most successful in similar cases, namely, sulphate of quina and opium, offered but a feeble hope of arresting the malady. It failed, as we expected, and the man died yesterday, worn out by long suffering and exhaustion. Eighteen hours after death we made a most careful examination of all the viscera of the three great cavities; not a single organ exhibited the least mark of inflammation; we could not find anywhere even the slightest trace of local congestion. The man had all his viscera in an apparently sound and normal condition, and died of pure nervous fever.

Some persons look upon the existence of fever independent of topical affections as purely imaginary, and deem those who have recorded such forms of disease as too ignorant or too lazy to make the necessary pathological investigations. I have not time at present to enter into this subject, but of nothing am I more convinced, than that fever may exist without any appreciable local lesion, that it may affect every organ and every tissue of the body alike, and yet that the most accurate symptomatologist cannot put his finger on any one single part and say, here is local inflammation of a decided character. I have met with many instances confirmatory of this fact in hospital practice. I recollect a case which occurred some time ago at this hospital, which was equally as remarkable for its extraordinary duration, as for the total absence of anything like visceral lesion. The patient was admitted into the small fever ward, labouring under an attack of nervous fever; he had thirst, hot skin, pulse from 110 to 120, occasional delirium and watchfulness, and these symptoms went on week after week, and month after month, unaccompanied, during the whole course of the disease, by any phenomena indicating the existence of local inflammation. His treatment was purely expectant and temporizing; we had no cerebral, abdominal, or thoracic lesion to combat; there was no organ in which the febrile derangement could be said to have fixed itself exclusively, no threatening disorganization calling for the employment of prompt, new, and energetic means. At last, after the fever had continued for very nearly three months, the man complaining all the time of more or less thirst, hot skin, watchfulness, and headache, with occasional delirium, the disease terminated in a well-marked crisis, accompanied by sweating. He fell asleep, began to perspire, awoke with a pulse nearly reduced to the natural standard, and perfectly recovered. I may observe that I have never seen fever last so long as this, nor have I ever observed a perfect crisis in any case after the forty-second day. Some time ago I attended the brother of a gentleman now present, who had a long and very severe attack of fever; though he never had a remission during his illness, and was in very urgent danger, he got a perfect crisis with profuse perspiration on the forty-second day, and is now in the enjoyment of excellent health.

You perceive, then, that the case of Lynam presents some circumstances worthy of notice. His fever went on to its termination without any symptoms of inflammation in any viscus, and his actual condition, as carefully ascertained by an accurate post-mortem examination, affords a useful lesson to the pathologist. His case is also interesting as showing how previous habits will modify in a remarkable degree the character of fever; for in him you have seen fever combined with the phenomena of delirium tremens, a state of things which it was natural to expect in a man of extremely intemperate habits. The termination of the erysipelas without any sign of disorganization within the cranium is also worthy of notice. In such cases you have it frequently followed by inflammation of the brain and its membranes, and an exudation of pus on the surface of one or both the hemispheres; but here you perceive that there has been no extension of the disease, nor anything that should have induced us to give up the plan of treatment we adopted, and direct our therapeutic means to the head.

There is another man, named Vero, in the fever ward, whose case I beg you will study with attention. He applied for admission here some time ago, labouring under violent and general bronchitis, accompanied by high inflammatory fever; we took him in at the time, as his case was one of the most urgent danger, but were obliged, by the crowded state of the hospital, to put him into the large fever ward. It is unnecessary for me to detail the treatment employed, as you have all witnessed it. By the most energetic measures, we succeeded in arresting the disease, but his convalescence was rendered tedious in consequence of his having been suddenly affected by a small quantity of mercury. His mouth became very sore, his breath fœtid, his gums spongy, the inside of his lips covered with lymph, and his system exhibited all the marks of mercurial irritation; but, under the care of Mr. Grady, a gradual but decided improvement in his condition was going on, and he was advancing rapidly in convalescence, when, unluckily for himself, he was persuaded to leave the hospital for the sake of voting at the city of Dublin election. In doing this he was necessarily much fatigued, and was exposed to cold on returning from the heated booth.

Now, mark the consequences of this indiscretion. This man just arrived at the period of convalescence from a severe and dangerous inflammatory fever, and greatly debilitated both by the disease and the venesections and other remedies necessarily employed, improvidently exposes himself while his frame was still emaciated and weak, and while his mouth was still sore in consequence of severe mercurial salivation: in this condition he exposes himself to the operation of mental excitement, great bodily fatigue, and cold—and what have been the consequences? —Why, that a new attack of fever immediately struck him to the ground with a heavy hand, and, after an absence of ten days, he returned to the hospital on the 24th of January, complaining of rigors, and other symptoms indicative of commencing fever. We saw him next morning, that is, before this new fever had lasted more than twenty-four hours, and we found him affected in a most remarkable manner; we found him labouring under a number of severe symptoms, which would have led the most experienced, if asked to guess how long his fever had already lasted, into the commission of a gross error, for he would answer that it must be at least the eleventh day. It is, indeed, very rare to find fever at once commencing with symptoms such as we observed on the first day in Vero. Great prostration of strength, hot skin, dry tongue, pulse 108, nervous agitation, restlessness, together with subsultus tendinum, were present from the commencement. The subsultus was very remarkable, and increased to such a degree, even on the second day, that Mr. Grady found it very difficult to count the pulse at the wrist; and yet, though his muscular system was thus irregularly excited, and its nervous influence deranged, he had not even a tendency to delirium, and he slept soundly; neither had he the least headache.

I called your attention to this circumstance at the bed of the patient, and I endeavoured to impress strongly on your minds how forcibly this case opposes the doctrines of those who attribute all the nervous disturbance of every part of the system, and amongst the rest subsultus, to congestion or to inflammation of the brain. When the subsultus had attained to a degree of violence in Vero's case such as we seldom witness, we remarked, nevertheless, that he slept well, had a clear eye, without the least approach to suffusion, and that he was free from headache, heat of scalp, or throbbing of the temporal arteries. Neither were we able to detect the slightest indication of inflammation, or even of congestion, in the chest or abdomen. The breathing was indeed quickened, but only in proportion to the acceleration of the pulse, and there was no cough or thoracic pain or uneasiness. The belly was fallen, soft, and quite free from tenderness, and there were no griping pains, flatulence, nausea, or diarrhœa, and yet the patient was evidently very dangerously ill. Agitated with subsultus, he was in a constant state of restlessness when awake; his skin was hot, his tongue dry, and his weakness was sudden and excessive; in short, he was labouring under intense *nervous fever*. This is a rare form of disease, and one the very existence of which most modern pathologists have been in the habit of denying; but, as I told you before, I have seen several examples of it.

I may remark that, in the present epidemic fever,* the termination of the disease by a well-marked crisis never occurs. Now, in the epidemic fever of which I have spoken in a former lecture, and which committed such devastations in 1826, a crisis was observable in the majority of the cases, and was almost always preceded by rigors and a hot fit, attended for a few hours with marked exacerbation of the symptoms, and followed by a most profuse warm, general perspiration, bringing perfect relief, and often so excessive that the steam of it could be seen issuing forth in vapour through the blankets in which the patient lay wrapped. In the beginning of the epidemic, the critical rigor often took place on the fifth day, and oftener on the seventh, but, as the disease continued, these short fevers, which, by the bye, always left the patient very liable to relapse, entirely disappeared; and when the epidemic reached its acme, the crisis rarely took place so early as on the eleventh day, and most generally on the fourteenth or seventeenth day.

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* 1834—5.

You perceive that, in judging of the truth of the doctrines held by the ancients, concerning the existence of critical days in fevers, an observer of the present epidemic might be led into error, and might, by generalizing too hastily, arrive at the false conclusion that this doctrine of critical days is totally destitute of foundation. But, to return to our patient Vero, it is not very difficult to explain why, in him, the moment fever was excited it assumed the nervous type. He had been debilitated by severe inflammatory fever and by the active antiphlogistic treatment, and, above all, his nervous system had been severely tried by an unexpected mercurial salivation, brought on by an unusually small quantity of calomel.

You are aware that various nervous symptoms attended with irregular muscular action, and simulating chorea, or paralysis agitans, are frequently the result of metallic salts, whether lead or mercury. For this reason, I look upon the previous mercurialization as the chief cause of the nervous type of Vero's fever. In spite of all our efforts, he died exhausted on the tenth day.

As long as life lasts, no matter how fatal the symptoms may appear to be, you should never despair of recovery in fever. You will find many examples of recovery in the most hopeless cases in the lectures which I have given you on this disease, but I cannot forbear quoting the following striking illustration which occurred in the practice of Dr. Hudson of Navan. He consulted me as to the treatment during convalescence, and I shall read for you his report :—

"Miss B—— appears to have sickened about the 9th or 10th of June, 1844, but I did not see her until the 20th. She had then some very serious symptoms. She complained of extreme debility, had much subsultus, constant sweatings, diarrhœa and meteorism, and unusually severe headache. I ordered a few leeches to be applied behind the ears, and for some days endeavoured to keep the diarrhœa in check by small doses of hydrargyrum cum cretâ and Dover's powder. It increased, however, and I applied a blister over the cœcum, and gave acetate of lead until a check was given it. By this time (five or six days after my first visit) the head had become more seriously engaged. She had low muttering, lay on her back, had involuntary evacuations, &c., and diarrhœa set in more smartly than ever. I applied a blister to the nape of the neck, gave port wine in small quantities pretty frequently, and decoction of bark, with aromatic confection, and occasional doses of musk and camphor. I ceased giving acetate of lead by the mouth, and ordered an enema of four grains of the acetate and four drops of laudanum to be given on each return of the diarrhœa. This treatment gave it a final check, and though the poor patient's weakness was now extreme, still I had hopes that she would fight it out; but on the night of June 30th a fearful change came on. Cold skin, succeeded by heat and excessive greasy perspirations, laborious breathing with loud rales, fluttering pulse, at times imperceptible, &c., &c. I was sent for early on the following morning, and found her breathing loudly and hurriedly, with *stertor;* the eye fixed and glassy, pupils contracted to a point, face bloated and livid, loud rales throughout the chest. I found it not possible to arouse her to consciousness. The abdomen was swelled and tympanitic to an enormous extent. She had convulsive twitchings of the mouth, a commencing puff in the respiration. In fact, she seemed dying; and as the closing act of the fever seemed to be a sudden pulmonary congestion, I proposed to try the desperate chance of a bleeding, if only to gain a little time for further measures. I accordingly took away four ounces from the arm, and immediately applied sinapisms to the spine and feet, and relays of hot flannel, sprinkled with turpentine, to the belly, giving a few drops of the oil of turpentine in brandy punch. The turgescence and livid colour left the face after the bleeding, and never returned; but in any other respect, save that the breathing was a little easier, I did not possess at least the negative quality of doing no harm. They are most commonly prescribed in combination with a considerable quantity of fluid, and hence are administered either in the form of draught or mixture. The medicine in most general use among the physicians of Dublin is one which was introduced by Dr. Cheyne. It is prepared by dissolving a drachm of carbonate of ammonia in three ounces and a half of water, with as much lemon juice as will saturate it; the mixture is then sweetened with syrup of orange peel, and given in doses of two tablespoonfuls every third or fourth hour. In this way a solution of the citrate of ammonia is formed, which possesses the properties of a mild anti-febrile and gently stimulant diaphoretic.

Now it cannot be denied that this mixture answers the purpose of an expectant remedy, calculated to pass away the time, and do no injury; but it appears to labour under one considerable disadvantage, it is not agreeable to the taste. If you taste the citrate or acetate of ammonia, you will find that its flavour is by no means pleasant, and I need not tell you that, in cases where there is no actual indication to be fulfilled, it is of importance to have something that will not be disagreeable to the patient. Feeling, therefore, the necessity of altering this prescription, I have lately introduced another, which I am happy to find has been extensively adopted, and which is formed by substituting the carbonate of soda for the carbonate of ammonia. The mode in which I generally employ it is the following:—carbonate of soda, a drachm; water, four ounces; lemon juice, a sufficient quantity to saturate the alkali; syrup of orange peel, half an ounce; tincture of orange peel, two drachms. A little more than an ounce and a half of lemon juice will be sufficient to saturate this quantity of carbonate of soda, whereas it would take from two and a half to three ounces to saturate the same quantity of carbonate of ammonia. If you wish to have a weaker solution, and I believe it is the better way, you can dissolve a drachm of carbonate of soda in five ounces of water instead of four. Nothing can be more agreeable in flavour than this mixture. The citrate of soda which is formed, does not, it is true, exert any active influence on the animal economy, but it partakes in the properties of neutral salts, determines gently to the kidneys, tends to keep up a soluble state of the bowels, and forms a most grateful and refreshing beverage. The syrup of orange peel gives the mixture an extremely pleasant flavour, and this is further heightened by the agreeable aromatic bitter of the tincture. Since I commenced using it, I have found it to answer all the necessary purposes extremely well, and I can recommend it to you with great confidence.

A woman named Anne Scarlet was admitted on Saturday, concerning whose case it may be necessary to make a few observations. She states that she has been ill for the last eight days, and that her illness originated in cold, preceded by rigors, and followed by feverish symptoms. The general pyrexia had subsided at the period of her admission; but she had some symptoms worthy of attention. Her pulse was 72, and regular; her skin rather cool, and her bowels natural; but she complained of acute pain in the left side, which, she said, came now and then, catching her breath, and preventing her from taking a full inspiration. This pain was so intense, and seemed to affect respiration so considerably, that, looking to its situation and its effects, you would at first sight be inclined to think that it arose either from pleurisy or pericarditis. On examining the chest, however, by the stethoscope and percussion, we found the sound was clear and normal: there were no rales present, and the respiratory murmur was heard distinctly over the whole lung. In fact, auscultation showed that the cause of the pain was not connected with pleuritis, pneumonia, or pericarditis. What then was it? A variety of pleurodynia, well worthy of your attention as being connected in her case with retention of the milk and engorgement of the left mamma. At the time she was attacked with cold, she happened to be only a few days after childbirth; the feverishness which ensued obliged her to give up nursing, and in this way a sudden and unnatural check was put upon the secretion of milk. When an occurrence of this kind takes place, and proper means are not taken to obviate the mischief, a high degree of local irritation is the consequence, producing inflammation of one or both the mammæ, which, if not treated well and energetically, will certainly end in mammary abscess.

What I wish to draw your attention to, however, at present, is this—that inflammation of the mamma, arising from retention of milk, is very apt to be attended with pleurodynia in one or more parts of the chest. The flow of milk to the breasts, three or four days after delivery, is very often accompanied by flying pleurodynia; and the formation of mammary inflammation, from the arrest of lacteal secretion, is also very frequently attended with fixed pains of a pleuritic character.

The treatment adopted in this case was very simple. In the first place, you endeavour to check the determination of fluid to the breast; and for this purpose you exhibit a purgative of a hydragogue kind, calculated to act briskly on the bowels. We gave a combination of infusion of senna, sulphate of magnesia, tincture of senna, and electuary of scammony, which acted six or seven times on the bowels, and tended materially to relieve, by derivation, the mammary congestion. In the next place, we directed our attention to the breast, and endeavoured to remove the milk by the use of the syringe employed for that purpose. The milk may be removed from the breast by means of the syringe, or by sucking with a breast-bottle, and where the tenderness of the part is so great that neither of these modes can be employed, the next best means is diligent fomentation. This produces a constant oozing from the breast, and if the fomentation employed be made with a decoction of poppy-heads, it has considerable effect in abating pain and inflammation. We also applied leeches in this case, not with the view of removing the pleurodynia, but with the intention of removing its cause, mammary inflammation. By the use of means directed to the breast, you will find that we can remove all symptoms of pleurodynia, and that the pain and difficulty of breathing will soon disappear. This is a simple case, but it is one of frequent occurrence, and it requires some tact and management for its successful treatment.

You have probably observed that, in the treatment of all the cases of fever that came before me, I have not prescribed altogether a dozen grains of calomel, that I have very seldom ordered any kind of purgative medicine, that I have been sparing in the use of leeches and cupping, and that I have not ordered a single patient to be blooded. This I am sure will appear strange to the various sects of pathologists and theorists whom I have seen, like so many waves succeeding each other, and whose doctrines were equally doomed to break on the solid and immovable shore of truth. I recollect how each doctrine arose, and made converts, and influenced practice; how each had its day, and then sank into that obscurity and neglect to which vain and profitless speculations are always destined.

I recollect when it was the custom to commence the treatment of fever by prescribing ten grains of calomel, to be followed by a bolus containing fifteen grains of jalap, or by a large draught composed of infusion of senna, Epsom salts, and electuary of scammony. I remember the time when it was the fashion to bleed every case of fever which came into hospital, no matter what the stage of the disease might be, or what the condition of the patient was at the time of admission. I recollect, too, when the prostration and weakness which accompanies local inflammation, particularly of the digestive system, used to be treated with wine and stimulants.

Every epidemic is peculiar and distinct in its nature, and each consequently requires a distinct and peculiar mode of treatment. Hence the necessity of studying fever unbiassed by any preconceived notions, and independent of the trammels of dogmatism. With a person who observes in this way, who studies the disease as it is, and not as it is described; whose practice is regulated, not by the doctrines of the schools, but by the results of investigation, carefully weighed and considered; with such a person, the treatment of fever will be simple and successful, and I believe that there is no disease in which success so much depends on treatment as fever. It is difficult to explain how it came to pass that a contrary opinion could be promulgated in Dublin. Something must be attributed to the neglect or incapacity of those whose duty it was to teach the truth.

The chief cause may, however, be traced to the activity and zeal which inspired some, not only to uphold their own branch of the profession, but to decry, I had almost said to defame, that which they were pleased to call *pure medicine*. With characteristic inconsistence, however, these gentlemen, who declared that the treatment of fever was at best useless, readily engaged in its management in private practice, and while they professed openly their disbelief in the efficacy of any medicines, they busily employed themselves in prescribing pills and draughts without number for their own fever patients. That they thought their treatment of some value might be gathered from their acceptance, their invariable acceptance, of pecuniary remuneration from the sufferer's grateful friends, who little dreamed the while that the hands which, with automatic movement, so readily grasped their fees, belonged to persons who held, nay, who maintained, the opinion that the treatment of fever was all a farce. Posterity will scarcely give credence to this fact, and will probably refuse to believe that such an opinion could have been advanced in what we are pleased to call an enlightened age, and an enlightened city. They will scarcely think I speak the truth in assuring them, that a spirit of medical intolerance existed to such a degree at the time of the discovery of the stethoscope, that whoever in Dublin actively occupied himself in verifying the researches of the immortal Laennec, whoever availed himself of the new resources invented by this great physician, was sure to become an object, not merely of dislike, but of animadversion and ridicule, on the part of those who ought to have exerted their influence in endeavouring to advance, and not to retard, the progress of science. Happily for the character of the country, their endeavours have been frustrated, and the cause of truth has triumphed. Happily for the students and their future patients, those teachers are now most followed who best explain and most diligently illustrate the phenomena observed by means of mediate auscultation.

LECTURE XXI.

YELLOW FEVER OF THE BRITISH ISLANDS.

In the epidemic of 1826, we had several cases in the Meath Hospital that presented all the characters of yellow fever. This is a very remarkable fact, for this form of fever has been very rarely witnessed in this country, nor does it seem to have been observed in any part of Europe to the north of Cadiz, Gibraltar, and other towns of Andalusia. The last epidemic of yellow fever in Gibraltar is described by Louis, who with Trousseau and Cherrin was sent by the French Government to investigate the disease. I shall quote from his report an account of the symptoms and post-mortem appearances which they observed, and compare it with the description of the yellow fever of 1826, printed by myself and Dr. Stokes, for the use of the pupils of the Meath Hospital.

"The disease," says Louis, "spared neither age nor sex; men and women, young and old, were alike its subjects. Those only were exempt from its influence who had been affected with the disease in a former epidemic.

"It commenced at different hours of the day, sometimes in the night, sometimes fasting or soon after eating, usually with an intense headache, accompanied by chills, shivering, pain in the limbs, and soon after pain in the back. A heat, rarely intense, succeeded to the chills, and was sometimes followed by perspiration. At the same time the countenance became red and animated; and in some cases, as it were, swollen. The eyes were red, glistening, suffused, and in many cases the patients complained of a smarting sensation in them. The thirst was intense, the anorexy complete. It was rare that the patient suffered any pain in the epigastrium at this period.

"The first symptoms, the headache, the pain in the limbs, the anorexy, the thirst, the heat, the redness, and the pain in the eyes, continued; the headache during half the disease, the pains in the limbs a little longer, and the heat, which in many cases was but slightly increased, continued so during nearly the same time.

"The pain in the epigastrium, so rare at the commencement, came on usually fifteen or twenty hours later. It was generally inconsiderable, and very few patients complained of severe or acute pain. With the epigastric pain came the nausea and vomitings, excited by drinks and purgatives in several cases, spontaneous in others. The dejections were infrequent, that is where no laxatives had been administered. The abdomen preserved its form, was supple and indolent, except in the epigastric region. The sleep was inconsiderable—some patients were restless, in some there was a good deal of jactitation during the night; others, and the smaller number, experienced, as early as the third day, a real anxiety, could not remain quiet in any posture, and in some cases there was delirium. But this symptom did not usually come on till the last day of life; and for this reason it is to be considered rather as belonging to the agony than to the disease; otherwise, with few exceptions, there was neither prostration nor stupor. The pulse was moderately accelerated, regular, generally bearing relation to the degree of heat, which was almost always slight, as I have before said. The skin of the thorax was injected in some cases. This redness and that of the eyes diminished toward the middle period of the disease, or a little later, and new symptoms appeared. To the injection of the integuments of the chest there succeeded a slight yellow tint of that part, and the eyes were the same colour. When this colour appeared thirty-six or forty-eight hours before death, it became rapidly brighter, so as to be of considerable intensity at the time of the fatal termination. In other cases where it came on only just before death, it was slight at the autopsy, and commonly limited to the trunk. At about the same period, or a little later, the matter vomited and the discharges from the bowels, which up to that time had presented nothing remarkable, took on a certain character, which they have not in the course of the acute diseases of Paris. The dejections were blackish or bluish, and the matter vomited, from being of a yellow colour, became brown or black. At the commencement of this change of colour, the vomit was a liquid matter, more or less greyish, mixed with a greater or less quantity of mucus, in which were to be seen blackish particles, like soot.

"At this period of the disease, the uncomfortable feelings and the anxiety continued during different lengths of time, and in different degrees, the strength diminished, the temperature fell, so that the limbs were cold before the agony; in a certain number of cases there was a suppression of urine. Sometimes also we observed a sort of remission, an apparent amelioration of the symptoms, and death took place when it would least have been expected, had not experience taught us to distrust this deceitful remission. In some subjects the violence of the headache, that of the pains of the limbs, the marked febrile symptoms, the numerous vomitings, the uncomfortable feelings, the anxiety, the bright redness of the eyes, gave to the disease a truly serious aspect; whilst in others the mildness of the fever, and of the pains wherever seated, the absence of agitation and delirium, the slight diminution of the strength, impressed on the disease a character of mildness, calculated to deceive at once the patients, their attendants, and the physician. It is under this form of the disease that patients died without taking to their beds—on foot, as it was expressed by their friends. Thus Dr. Mathias, who died after an illness of four or five days, experienced no other symptoms but severe pains in the calves of the legs, and a suppression of urine. He had no nausea; he did not vomit; his mind was perfectly clear during the whole course of the disease.

"This kind of latent condition of the yellow fever does not distinguish it from the acute diseases of Paris, which also are often obscure, and their symptoms mild; but it is remarkable on account of the rapid progress of the disease, usually fatal from the fourth to the sixth day. And this latent form reminds us at once of certain facts of poisoning by arsenic, in instances of individuals who have retained their clearness and calmness of mind from the moment of swallowing the poison until their death.

"I add, that the severity of the symptoms does not correspond always with that of the lesions. Of these last, one only was constant, the specific alteration of the liver. The inflammatory state of the mucous membrane of the stomach comes next in frequency, and sometimes explains in a manner sufficiently satisfactory the symptoms that had been observed."

The following are the appearances which Louis found in the fatal cases of yellow fever at Gibraltar* :—

"The stomach was larger than natural in seven subjects, smaller than usual in three. It contained a clear or dark red coloured liquid, a blackish or perfectly black fluid, in different quantities, in three-quarters of the cases. Its mucous membrane was red, through a greater or less extent, in six cases; rose coloured or orange in eight cases; greyish, yellowish, or whitish in the others. It was thickened through a greater or less extent of surface in half the cases; softened and yellow to an extreme degree in the same number; at the same time thickened, softened, and red in a third part of the cases; mamelonated in two-thirds; ulcerated in two cases; and natural in five cases.

"The mucous membrane of the duodenum was red in a little more than half of the cases; softened in the same number; and thickened in one case.

"The small intestines contained a greater or less quantity of reddish, brownish, blackish, or perfectly black matter, in two-thirds of the cases. Its mucous membrane was slightly injected or red in spaces, in a little less than half the cases. Its consistence was more or less diminished through its whole length, or through a part of its extent only, in rather a greater number of cases. It was partially thickened in one case; in no case was it ulcerated; and Peyer's glands were always natural.

"The large intestine was of greater size than usual in two cases. In fifteen cases it contained a matter of a wine lees colour, or blackish, or brownish, or chocolate-coloured, or entirely black. Its mucous membrane was of a pale or bright red colour in five cases; greyish, yellowish, or whitish in the others. Its consistence was more or less diminished in three-fourths of the subjects. Its thickness was increased in three cases; and twice we found it slightly ulcerated.

"The mesenteric glands presented traces of inflammation in four cases; the cervical glands in one case; in another case one of the glands above the *biliary ducts* was red, softened, and very large.

"The *liver* was of greater size than natural in two cases; a little firmer than usual in three cases; a little less firm in three others. Its cohesion was increased in six cases, diminished in seven. *Its colour was altered in every case; sometimes it was of the colour of fresh butter*, sometimes of a *straw yellow*, a *clear coffee and milk colour*, sometimes *a gum yellow*, sometimes *of an orange colour.*

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* I have intentionally omitted the very minute description of the thoracic viscera, the brain, spinal cord, &c.; suffice it to say, there was nothing observed worthy of note.

"The spleen was softened in eight cases, and to a moderate degree, with one exception. It was larger than usual in five cases.

"The lesions which we have thus placed before the reader *were rarely considerable, very often insufficient to explain the death*, and when this explanation was afforded, it was by a combination of several lesions.

"These lesions may be divided into two classes, some of them peculiar, or almost exclusively peculiar, to subjects dying of yellow fever; others common to those subjects, and to subjects who have died of other acute diseases. The red or black matter found in the alimentary canal, and the remarkable alteration of the liver, are of the first class, all the other lesions of the second.

"The red or black matter of the stomach or intestines not having been found in all the cases of yellow fever, it cannot be considered an anatomical character of the disease. But it is *not so with the alteration of the liver, which was more or less exactly the same in all the cases*, and which, for that reason, ought to be considered as the *essential anatomical character* of the yellow fever of Gibraltar of 1828.

"Amongst the lesions of the second class, the yellowness and the inflammation of the mucous membrane of the stomach should be especially remarked, as well from their frequency as on account of the rapidity with which they come on. The inflammation of the mucous membrane of the stomach not having taken place in all the cases, and Peyer's glands not having ceased to be natural, it follows on the one hand, that the yellow fever of Gibraltar, of 1828, is not a gastritis, and on the other hand that it is not a typhoid fever. This last conclusion is even more strict; for not only was there an absence of the lesions of typhoid fever in the bodies of the victims of yellow fever, but these bodies presented other lesions which are not found in the victims of the first disease, and which are peculiar to the second disorder.

"What, then, is the nature of the yellow fever of Gibraltar, of 1828, and where is the seat of it? If it be neither a gastritis nor a typhoid fever, neither is it a hemorrhage, as it has lately been said to be, for the hemorrhage did not take place in all cases. Is it a disease of the liver? Undoubtedly the liver was the organ principally and essentially affected; still we cannot regard the yellow fever as simply a disease of the liver, because its lesion, at least in the present condition of science, does not explain the febrile symptoms in the cases where this was the only lesion; and in the second place, because it is entirely insufficient to explain the death.

"As, then, a strict analysis of the anatomical appearances of the yellow fever of Gibraltar of 1828, proves the existence of a cause unequal in its operation, and of which but one effect is constant, the specific alteration of the liver, and, as in a third part of the cases, it is directly to this cause that we are obliged to refer the death, we naturally ask how does this act, through the medium of what system does it exert its influence on the economy? Is it through the nervous system; is it through the blood, in which, however, we have not detected any especial modifications?"

Let us now compare with this description the epidemic I witnessed in Dublin in 1826. The first case I shall speak of is that of John Gall, aged 35. Admitted about the 10th of January. Date of illness unknown; probably about seven or eight days. Tenderness of epigastrium chief symptom, and with it costiveness; skin hot; tongue very dry and brown in centre, edges white, a little moist; much debility; appeared stupid, but no delirium; memory uncertain; at one time he said he was two days ill; at another, for several; belly hard, full.—*Leeches to epigastrium, and purgatives with apparent relief.*—Next day he got *effervescing draughts*, and began to complain of cough.—*Blister on the chest on the following day.*—That night he became yellow, being convulsed in the abdominal muscles, and died at 5 a.m. yesterday.

Dissection 30 hours after death.—Body well made, strong, muscular; skin and conjunctivæ yellow; posterior parts livid. Dura mater yellow; no fluid between dura mater and arachnoid; considerable quantity of fluid under arachnoid, between convolutions, of amber-yellow colour; brain remarkably firm; substance white; yellow fluid in right ventricle, and also in left, in anterior cornua in considerable abundance, particularly in left. Abdomen: liver natural; no obstructions in ducts; bile in gall-bladder; stomach of a dark purple colour universally; mucous membrane increased in thickness, bleeds when torn, is evidently a little softened, villous coat like velvet; when in water, villosities whitish and floating. Near the pylorus we observed a very curious and beautiful appearance; the mucous membrane was here, as in other parts, of a purplish-red colour, marked in many places by rings of a white colour, and perfectly circular, and about half an inch in diameter. These rings, formed by a circle about half a line in breadth, included a space purple like the rest of the mucous membrane; and in many places intersections of these white circles were observed: white serpentine lines were also apparent in this part of the stomach. On placing the stomach in water, we discovered that these white circles and serpentine lines were formed by the extremities of villous processes, which had not a purple colour like the rest. Duodenum was also red, but the redness decreased gradually. *One intus-susception, including a portion of intestine six inches in length, was found in the small intestines. The invaginated portion of intestine was easily withdrawn from within that which had enclosed it, and there was not the slightest mark of inflammation in either.*

This is a good example of the morbid appearances exhibited by those fatal cases of fever which had been so frequent in the epidemic of 1826. We lost nearly twenty patients, in whom the symptoms ran nearly the course above detailed. In all, the abdomen became hard and tender about the epigastrium and hypochondria, and often without any premonitory symptoms indicative of the approaching danger. This hardness and *knotted* feel of the abdominal muscles was followed by an appearance of *general jaundice of a bright yellow colour*, accompanied by uneasiness and anxiety of countenance, a very quick and hurried pulse, and coldness of extremities. Death generally took place in such cases within twenty-four hours from the appearance of the jaundice, and was preceded in some cases by general convulsions (as was reported, but we ourselves did not observe any general convulsions); in most, by spasms limited to the abdomen, and which obtained among the nurses the appropriate name of "*Twisting of the guts,*" a name which agrees singularly with the intestinal intus-susceptions found in almost all. Before entering into the subject of the pathology of this singular form of fever, I shall detail a few more examples of it.

John Rochford, aged 50, was admitted into shed No. 4, with low fever; became convalescent after a few days without any regular crisis; appetite returned, and he continued well for about six days, when he relapsed. Belly a little hard, and tender to the touch; complained only of costiveness; some purgative medicine was directed, which not having the desired effect, an oil draught was prescribed, and having operated freely, patient appeared much relieved at next visit; but between 11 and 12 p.m. was seized with convulsions of belly, but not of extremities; suddenly became jaundiced, and died next morning. The tip of his nose became of a deep purple colour. The friends having taken away the body, we had not an opportunity of examining the morbid appearances.

We have had several cases in which the nose became purple in fever, and, with one exception, they all proved fatal. When the purple nose is combined with general jaundice, the patient presents a truly frightful appearance; this has happened in five or six instances. Sometimes the purple colour is limited to the tip of the nose, while in other cases it spreads from the nose to the upper portion of the cheeks. The parts about to become purple assume at first a pale appearance; this paleness is gradually converted into a livid leaden hue; and the part becomes quite purple generally in the course of twelve or twenty-four hours. It is to be observed that the parts thus affected preserve their natural heat until shortly before death, when, of course, the tip of the nose is among the first parts to grow cold. In the case of a girl in shed No. 2, whose nose and cheeks became purple, this change took place more slowly than usual. At first the parts were observed to be covered with broad patches of a wax-like whiteness, somewhat elevated above the surrounding surface, which so much resembled urticaria that it was considered to approach, in its nature, to that eruption; the following day, however, these spots were found to have become of a red colour, and on the next day the redness was converted into a deep purple. During the whole of this time the heat of these parts was not less than that of the rest of the body. She died on the following day.

In the case of a woman, also in shed No. 2, in whom the tip of the nose and the ends of some of the toes became purple, these parts were tender to the touch; this woman recovered. Leeches were applied to the tip of the nose, and tepid stupes or poultices kept constantly applied to the discoloured parts; a small portion of the nose separated and came away in the form of a slough. These facts prove that this purple colour of the nose and other parts, *in many instances, at least,* arises from a condition of the vascular system of these parts closely allied to inflammation. We possess a drawing of a patient in whom, from the effects of cold, the tops of the fingers became purple and excessively tender when exposed even to the common temperature of the wards in winter. Great relief from pain, and some diminution of intensity in the colour was obtained by keeping the fingers immersed in tepid water. This case, which was treated by Mr. M'Namara, had lasted for some weeks before admission, and yielded, but not until the lapse of a considerable time, to the employment of tepid applications, &c.

Patrick Mahon, aged 45, a stone cutter, strong habit. Admitted into shed No. 4, labouring under fever of a typhoid character. Tongue loaded; teeth covered with sordes; abdomen hard; tenderness of epigastrium, and hypochondria on pressure; complained of weakness.—*Twenty leeches were applied to the epigastrium, and purgative injections administered.* The following morning the skin and conjunctiva appeared slightly yellow; abdomen still hard; pulse weak and quick; much debility.—*Was ordered some blue pill, and to repeat the injections.*—At the next visit, the yellow colour continuing, the abdomen being still hard, and the epigastrium tender, twenty leeches were again applied, and the former medicines repeated. At four o'clock in the evening was seized with convulsions, and died early next morning. The convulsions only appeared to affect the abdomen. —Body not examined.

John Gaven, aged 22. This man's case differed in no material circumstances from the preceding cases.—*Dissection twenty hours after death.* Body extremely well made, strong, and muscular. Nothing morbid in head or thorax, except dilatation of some bronchial tubes. Abdomen: *five intus-susceptions in small intestines, without any adhesion or marks of recent inflammation;* other parts of the intestines considerably contracted; mucous membrane of stomach, from cardiac orifice to within about two inches of the pylorus, of a brownish-red colour. Here the mucous membrane yields readily to the back of the knife, and may be scraped off in a semi-fluid state; it contains several patches of ecchymosis. The whole of the intestinal tube, with the exception of the duodenum and the lower half of the large intestines, has its mucous membrane of a dark red colour, with numerous ramifications of vessels engorged with blood. In many parts the mucous membrane is very soft, and almost semi-fluid. *Liver perfectly healthy: no obstruction in gall-ducts.*

As time will not permit me to detail more dissections of this truly curious and fatal form of fever, I shall merely sum up some of the principal points connected with its pathology. 1st. In none did we find inflammation of the liver, or obstruction of the gall-ducts. 2ndly. In *all*, evident marks of inflammation were found in the mucous membrane of the stomach, such as redness, softness, &c. 3rdly. In almost every instance, we found one or more intus-susceptions in the small intestines. 4thly. All these were without any mark of inflammation of the serous membrane, and the invaginated portion of the intestine could be always easily drawn out of the other. 5thly. In *several* we found effusion of a yellowish or amber coloured fluid between the arachnoid and pia mater, at the base of the brain, and sometimes in the ventricles, but in these only in small quantity. 6thly. In none did we find inflammation of the brain or its membranes. 7thly. We found the spleen very much enlarged in almost all. When the spleen, in acute diseases, is thus engorged and distended, it is invariably softer than natural. In but one case did we find a considerable quantity of a dark red fluid in the stomach, together with a good deal of a substance resembling coffee grounds, and in this case the mucous coat of the stomach was in many places of a very dark colour, and a slimy consistence, so that there could be but little doubt concerning the origin of the contained fluid, and the coffee grounds substance, which must have proceeded from the diseased and almost disorganized mucous membrane. Such have been the principal appearances observed during the dissection of about fifteen fatal cases of fever combined with yellowness of the skin. The following cases will convey a more exact idea of the symptoms which characterize this form of fever than those already related, which proved too suddenly fatal to allow a full development of the symptoms.

Peter Kelly, aged 28, on the 29th December was admitted into No. 4 fever shed; stated that for two days previously he had severe cough without expectoration; pulse 110, strong; face flushed; tongue white, moist; pain across forehead, and general distress; great tenderness of epigastrium and right hypochondrium; costive; thirsty; abdomen hard: on examination no morbid rale was perceptible; respiratory murmur natural.—30th Dec. *Venæsectio ad* ℥xv. *Hirudines* xx. *epigastrio. Pilulæ purgantes et mistura purgans.*—31st. Cough very severe. *Vesicatorium pectori. Mistura pectoralis.*—1st January, 1827. During last night became jaundiced; considerable distress this morning; black stools; great tenderness of epigastrium and right hypochondrium; cough very troublesome. *Venæsectio ad* ℥xii. *Hirudines* xxx. *hypochondrio et epigastrio. Abradantur capilli et applicetur vesicatorium vertici. Sumat omni horâ calomelanos grana duo.*—2nd January. Much relieved; skin not nearly so yellow; tenderness greatly diminished; some sweat last night. *Repetantur pilulæ.*—3rd. Considerably improved; skin nearly natural.—4th. Mouth affected with mercury; skin natural. *Omittantur medicamenta.* — 5th. Removed to convalescent ward.—7th. Convalescence continues, having now no complaint but slight soreness of mouth.

Here the yellow colour appeared about the fifth day, and sweat attended with much relief on the seventh day. The symptoms chiefly worthy of notice are, the violence of the febrile reaction, pain of forehead, great tenderness of epigastrium and right hypochondrium; blackness of stools, and hardness of the belly. We shall just now see the great importance of these symptoms in determining the true nature of the disease.

January 14th.—Thomas Kearney, aged 38, labourer; has been ill for eight days; was first attacked with rigor and pains of loins and limbs, which still continue. He also complains of cough and pain of chest; head first attacked on fifth day; was taken into hospital the following day; got some purgative, which operated powerfully. Present symptoms: skin dry and hot; eyes and skin *yellow;* great pain of head; tongue dry and white; pulse 60; the colour of stools very dark; epigastrium tender.—January 15th. *Applicentur hirudines* xx. *epigastrio et vesicatorium pectori.* ℞. *Pilulæ hydrargyri,* gr. ix.; *extracti hyoscyami,* gr. vi.; *M. in pilulas tres divide; sumat* i. *ter in die.*

Habeat haustus effervescentes cum carbonate ammoniæ, et enema emolliens vespere.—Jan. 16. Pain of chest and cough removed, and pain of epigastrium diminished since the application of leeches, which still continue bleeding; ordered to be stopped by the application of caustic; tongue moist, looking like mercurial ointment; pulse 60, strong; countenance much improved; stools much more natural; yellowness nearly gone; sweated much. *Repetantur pilulæ hydrargyri et extracti hyoscyami.*—January 17th. No fever; yellow colour quite gone; many loose stools. *Omittantur medicamenta.*—January 18th. Convalescent.

The state of the pulse in this case was remarkable. It did not exceed 60 at a time when the existence of many other symptoms left no doubt of the febrile and inflammatory nature of the complaint.

December 30th, 1826. Esther M'Quillan, aged 38. Complains of general pains; has been subject to violent pains for the last three years, after having laboured under fever in Cork Street Hospital; was there also about four months ago, and was discharged cured. Present state: great headache; tongue brown in centre; pulse small and weak; great tenderness of abdomen on pressure; bowels very free; blooded last night for cough and stuffing of chest; finds herself much relieved; blood slightly buffed, no separation of serum; respiratory murmur natural; complains of pain across her back. *Applicentur hirudines* xx. *epigastrio.*—December 31st. Tongue parched, furred, and brown in centre; tenderness of epigastrium still remains, but much diminished; is very slightly jaundiced; leech-bites bled well; pulse 100, regular; great thirst; pains of joints and small of back excessive, and preventing motion in bed; breathing free; urine very light coloured. ℞. *Nitratis potassæ*, ʒij.; *decocti hordei*, lb. ij.; *acidi nitrici diluti*, ʒj.; *misce; consumatur in die.*—January 1st, 1827. Colour more yellow; great tenderness of epigastrium and right hypochondrium; pains as before; fever unabated. *Habeat calomelanos* gr. iij.; *opii* gr. ¼, *ter in die; Mistura camphoræ* ℥j. *ter in die.*—January 2nd. Pulse 72, weak, at times almost imperceptible, but regular; respiration easy; yellow stools, passed under her; belly very tense; abdominal muscles contracted and hard; tongue black and parched; raves, but is sensible when spoken to; lies on side. *Repetantur pilulæ et mistura; applicetur* *vesicatorium hypochondrio; vini* ℥vi.—January 3rd. A good deal of cough; raves continually; yellowishness deeper; many yellowish stools passed under her; debility much increased; thirst continues; tongue black and parched; heat natural; tremor; pulse 84; blister rose but little; deglutition impeded by a spasm; just before visit was seized with a fit, attended with spasms and rigidity of joints, which lasted about a minute; feet cold. *Vini rubri* ℥vj.; *applicentur sinapismi pedibus; repetatur mistura camphoræ.*—January 4th. Sensible when spoken to; puts out tongue when desired; but at all other times raving; seems to suffer extremely when joints are moved; frequent tremor and shuddering; rested scarcely any; other symptoms as yesterday; some swelling of ankles. *Applicentur vesicatoria suris; vini* ℥vj.—January 5th. Moaning and raving during the whole night; no vomiting; cough looser; slept a little this morning; stools yellow; tongue parched; blisters rose well; drinks abundantly; less yellowness; no headache; eyes suffused; pulse 84, scarcely to be felt, regular; no coldness of extremities; flatulence. *Repetatur vinum.*—January 6th. Slept well; no raving; countenance improving; fever much diminished in every respect.—January 7th. Tongue clean; pulse 80, stronger than before; a large purple spot, not elevated, occupies the whole of the outside of right instep; it is in some places vesicated; appetite good; slept well; smaller spots on other foot; camphorated spirit to be applied to spots. *Habeat sulphatis quinæ granum ter in die.*—January 8th. Edges of large spot more vesicated; whole surface has a redder and less purple colour; three stools during night; little sleep. *Repetantur pilulæ sulphatis quinæ et vinum.*—January 9th. Redness of right eye, not painful; foot better. *Applicentur hirudines* ii. *conjunctivæ.*—January 10th. Convalescent; remained for about a week, and was discharged cured.

The symptoms of this case were very alarming; so much so, indeed, that on the 2nd and 3rd of January we had little expectation of her recovery. At this period the involuntary discharge of stools, the extreme weakness of pulse, black parched tongue, general debility, raving, tremors, spasmodic affection, which supervened when she attempted to swallow, and finally, the *hard and knotted state of the abdominal muscles*, together with a fit of general tonic spasms; all these symptoms, combined with the yellow colour of the skin, rendered her recovery very improbable. The treatment was in the commencement antiphlogistic. The nitre was prescribed in order to relieve the rheumatic pains; but on the following day it was abandoned, and a preference given to calomel and opium for obvious reasons. Nitre does not act favourably in cases where much debility is present, or where the stomach is weak.

In several of the cases attended with jaundice which proved fatal, the symptoms were very similar to those described. The case of M'Quillan may be looked on as presenting a good example of this peculiar species of fever. *In her case, as well as in several of the fatal cases, the alvine discharges were of a healthy colour; and in several of the latter the bile found in the gall-bladder after death was in its properties quite natural.* About one-half of the persons so affected raved, betrayed great restlessness, and their countenance had a peculiar expression of anxiety; others seemed in perfect possession of their intellectual faculties to the last, but at the same time appeared in a most nervous, irritable, and desponding state of mind. They could not rest for a moment tranquil, but tossed their arms about, and regarded their attendant with a look expressive at once of *nervous* suffering and despair. Many vomited very often; all complained of extreme tenderness of the epigastrium.

Here I may observe that, in the epidemic of 1826, we opened many bodies in which peritoneal inflammation might have been expected, judging from the *extreme* epigastric and abdominal tenderness during life: and yet we found no marks of peritonitis whatsoever. The large *purple spot* in the instep seemed at first of a similar nature with the purple colour of the nose and lips before described; it proved, however, to be erysipelas, and ended in vesication. It differed from common erysipelas in its dark livid purple colour, and in having a well-defined abrupt boundary, and in the colour disappearing but little on pressure. In fact, it seemed to be as it were intermediate between purpura and erysipelas. The advantage of wine and stimulants towards the conclusion of this fever was very apparent.

January 15th, 1827. Robert Farmer, aged 19. Has been ill five days; was employed in a brewery, where he was exposed to hot steam, producing a copious perspiration, during which he drank a great quantity of cold beer; was immediately seized with a violent rigor and fulness of head; the rigor lasted for an hour; a comparative calm ensued. The head, however, still continued uneasy; loss of appetite followed; but he endeavoured to work for two or three days, when he was obliged to remain in bed; has been in a violent heat since, unless he gets a cold drink, which causes a rigor; was admitted into hospital yesterday. *Previous to this* had taken no medicine.

Present symptoms: violent pain or rather fulness of head; throbbing of temporal arteries; pulse 110; thorax free from pain; no cough; epigastrium and abdomen very tense; no tenderness on pressure; skin, hot, dry, and tinged yellow; tongue, white and dry, somewhat moist at edges; got some purgative medicine, which procured two stools, fœtid, and of a dark colour; urine natural. *Applicentur hirudines* xx. *temporibus.* ℞. *Liquoris acetatis ammoniæ, aquæ fontanæ, singulorum,* ℥iij.; *tartari emetici, granum; Syrupi* ℥j. *misce. sumat* ℥ss. *omni horâ. Habeat enema emolliens vespere.*—January 16th. Leeches were applied at 6 p.m.; many still bleeding; eyes and skin less yellow; headache less; pulse 70, regular; a slight tendency to diaphoresis. *Repentantur medicamenta ut heri.*—January 17th. Not much headache; heat and pulse natural; much debility; tongue clean and moist; countenance improved; no appetite; bowels free; convalescent.—January 26th. Left hospital the day before yesterday; and that evening experienced rigor and headache. Tongue white and furred; pulse 100; skin not very hot; abdomen soft; bowels free; great thirst; no headache at present. *Habeat haustus effervescentes cum carbonate ammoniæ.*—January 27th. Respirations 36; pulse 120; abdomen soft and natural: a good deal of headache; thirst; heat of skin; flushing of face; tongue as yesterday. *Applicentur hirudines* xx. *temporibus.*—January 28th. Head somewhat relieved; bled all night from leech-bites; much tenderness of epigastrium; pulse 125; great thirst; no vomiting; some yellowness of skin, but not of eyes. ℞. *Pilulæ hydrargyri*, gr. ix.; *extracti hyoscyami*, gr. vj. *misce; fiant pilulæ tres; sumat unam quartis horis.*—January 29th. Fever diminished; was extremely weak last night, and had great distention of belly, with swelling and tenderness; this attributed to taking too large quantities of drink; was relieved by a large oil injection three times repeated; very little yellowness to-day. *Habeat haustus effervescentes cum* *carbonate ammoniæ.*—January 31st. Skin hot; pulse 110, rather weak; all the symptoms exacerbated since yesterday; much thirst; tremor; no cough nor tenderness of belly; no headache nor raving; but little sleep; respirations 40; bowels free; much nausea, but no vomiting. *Habeat haustum oleosum; repetantur haustus effervescentes cum carbonate ammoniæ.*—February 1st. Face flushed; no headache; a good deal of epistaxis last night; dry burning heat of skin; tongue very red at tip and edges; parched in centre; vomited last night; much thirst; no tenderness of epigastrium; respirations 36; pulse 112; no cough; complains at times of sense of distention of stomach. *Habeat misturæ camphoræ cum magnesia,* ℥j. *ter in die.*—February 2nd. No fever; pulse 72. Convalesced slowly and was dismissed cured.

Here the crisis of the relapse was better marked than that of the first attack, and occurred on the ninth day of his relapse. One of the most prominent features of this fever was the distended state of the epigastric region, in the first attack unattended with tenderness, but in relapse accompanied by much epigastric tenderness. It is probable, therefore, that the distended state of the epigastric region proceeded in both instances from the same cause, namely, inflammation of the mucous membrane of the stomach. We have already seen that this inflammation may, and generally does produce very great tenderness; this case, however, seems to prove that inflammation of the mucous membrane of the stomach may occasionally exist without producing tenderness. We have found both the extract and tincture of hyoscyamus extremely useful in abating irritability and procuring sleep in the advanced stages of fever. In the fevers attended with jaundice, we were induced to combine it with mercurials, from observing the frequent occurrence of intus-susception in the fatal cases—still bearing in mind that means calculated to abate the inflammation of the stomach and intestines, by lessening the cause, would strike at the root of the spasm, and thus prove the best anti-spasmodics. This plan has been successful in several instances, but in the majority of the yellow cases, we regret to say that the progress of the disease was so sudden, most terminating in twenty-four hours after the appearance of the jaundice, that all our efforts proved ineffectual. In the second report we made will be found the history of the dissection of several of those cases which occurred after February. It is not to be supposed that the report affords specimens of all the varieties of fever treated during the time it embraces—we have omitted to detail any but those calculated to convey an accurate idea of the general character of the epidemic and its peculiarities, omitting any account of the more ordinary forms of maculated and typhus fever, which were not unfrequently observed. It concludes with some remarks on that form of fever which was accompanied by jaundice.

Cases of probably a similar nature have been observed by Dr. Cheyne and others in former epidemics, but in no other epidemic were they so frequent or so fatal in this city. Those who are familiar with the symptoms and morbid appearances observed in the yellow fever of America, the West Indies, and of Spain, will at once perceive many striking points of resemblance between yellow fever, properly so called, and that variety of fever we have described. In both the yellow colour depends upon the presence of bile, and in both the absorption of bile into the system seems independent of hepatic inflammation or obstruction in the biliary ducts. We are aware that Tommasini, in his excellent work upon the fever which occurred at Leghorn in 1804,* proves that the liver is inflamed not unfrequently in yellow fever, *and he supposes* that it is inflamed in all cases, arguing that where no very *visible* or *external* marks of hepatic inflammation have been observed, still inflammation may have existed in the internal parts of the liver, attacking chiefly its vascular system and the pori biliarii (*page* 239). As, however, no such inflammation to our knowledge has been detected in those cases of yellow fever which present an apparently healthy state of the liver, and as the most accurate descriptions of the morbid anatomy of yellow fever with which we are acquainted† report a healthy state of the liver in the majority of cases, we must, for the present at least, consider the jaundice of yellow fever as independent of hepatitis.

An inflamed state of the mucous membrane of the stomach, often amounting to its absolute disorganization, is the most constant and the most essential morbid appearance in yellow fever:—a similar state of the duodenum is likewise frequent; now in both these respects our cases agree with yellow fever, except indeed that in the latter the disorganization of the mucous membrane is greater; still, however, this is only a difference *in degree;* and in one of our cases we have seen that the disorganization of the mucous membrane was fully equal to that described in yellow fever attended with the black vomit; and in that case the stomach contained matter very similar to, if not absolutely identical with, the black vomit. We should recollect also, in comparing these two forms of disease together, that in many instances of yellow fever there is no black vomit, and the inflammation has in such persons been found to have attained a degree not greater than was observed in our cases. The tenderness of the epigastrium, so prominent a feature in yellow fever, occurred in all our patients; and, if space permitted, I could point out many other circumstances of similarity between these two forms of fever. It may appear to many ridiculous to maintain a similarity between these cases and yellow fever, a disease of warmer climates, and which commits such fearful ravages wherever it appears. I need, however, only refer to the works of Tommasini, Bancroft, Dr. James Johnson, Bartlett, and Clymer, which contain ample proofs that even in the warmest latitudes epidemics of yellow fevers are always mixed with fever of a bilious character, but of a milder type, a circumstance which renders it highly probable, that were such an epidemic influence at any time, from a particular combination of circumstances, to spread to temperate latitudes the reverse would happen, and this influence would then produce an epidemic of bilious or gastric character, with comparatively few cases approaching in violence to yellow fever.

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* Sulla Febbre di Livorno, e sulla Febbre Gialla, &c.

† See Laurence's very accurate Dissections of subjects dead of the Yellow Fever, made at New Orleans during the years 1817—18—19.—*Philadelphia Journal*, vol. i. new series.

Tommasini and the best modern pathologists consider it as now placed beyond all doubt that yellow fever cannot be considered as a specific disease, but merely as the maximum of bilious or gastric fever. By some it has been considered as a variety of *remittent*, but nearly all the late writers agree in regarding it as a *continued* fever.* In proportion to the warmth of the climate these fevers increase in intensity. Thus, in Cadiz and Gibraltar we need not be surprised at the occasional appearance of the yellow fever, approaching in violence to that of the southern parts of North America and the West Indies. At Leghorn the resemblance, although still striking, was not so perfect: and again, in the bilious epidemics of France, Holland, and Germany, the difference, as to intensity, is still greater (Tommasini, 81, 82, 83), but still the disease, in its essential characters, remains the same in all, and the same symptoms, and the same morbid lesions are found; they differ only in degree. Hitherto we have not made any remarks on the frequent occurrence of spasmodic action of the intestines, as proved by the intus-susceptions so constantly observed in our cases; *a circumstance, we believe, peculiar to those cases, for we have not met with any account of a similar occurrence in other epidemics.* How far such spasms, either by directly causing a temporary constriction of the ductus communis choledochus where it enters the intestine, or by extending to that duct itself, may have contributed to obstruct the passage of the bile and produce the jaundice, is a question worthy of consideration.

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* Clymer on Fevers, Philadelphia, 1846, p. 363.

On looking over my papers, I found the following notes of a clinical lecture delivered at the Meath Hospital in the year 1827. As they have especial reference to the subject under consideration, I shall make no apology for introducing them in this place, merely premising that they are printed as they stand in the manuscript, and must be regarded as the *heads* of a lecture, and not as conveying all that I may have said on the occasion:—

In fact there is not so much difference between the diseases of Ireland and warmer countries as has been imagined. They differ, it is true, as *to their degrees*, but not as to their pathology.

It was an opinion long ago advanced by the late Dr. Whitley Stokes, that almost all fevers pass into each other—thus, intermittent may become continued, and typhus fever, perhaps, but an inferior grade of plague. Be this as it may, each particular epidemic *has a grade peculiar to itself.* The present fever in Dublin, the most worthy of notice we have had here, strongly proves that our diseases differ only in degree from those of warmer latitudes. We have had numerous cases which in their symptoms and their morbid anatomy agree essentially with the *yellow fever.* This is an opinion I have never before expressed, as I was unwilling to do so until after careful examination and study. Passing events always make a deep impression when the attention is *properly directed towards them.* I stand here to improve you, if I can, in the pathology and practice of physic, and scruple not to deviate a little from my course, if that deviation be calculated to awaken your attention to an important subject.

1st. In both, patients become yellow from absorption of bile into the system; but observe, in epidemics of yellow fever it never happens that all, or even most, of the cases turn yellow.

2nd. These yellow cases are here equally fatal.

3rd. Tenderness of epigastrium, and vomiting in both.

4th. The strongest die.

5th. Jaundice does not depend on hepatitis in *either*.

6th. Nor on any *permanent* obstruction in gall ducts.

7th. In both, the essential character of the disease is a violent inflammation of the mucous membrane of stomach and duodenum; which becomes dark purple, soft, and semifluid.

8th. Black vomit in true yellow fever consists of a sanguineous fluid, the mixed vitiated secretion of the stomach and the blood forming the coffee-grounds appearance. This black vomit we found in one of our patient's stomach.

9th. Enlarged state of the spleen.

What is the cause of yellowness in yellow fever? Absorption of bile. But what causes bile to be impeded—there is no obstruction in the liver itself—as in hepatitis, or in the ducts—as in gall-stones, &c.?

It has been ascribed to *vomiting*, but this is not the true cause.

1st. It has appeared in yellow fever when there was no vomiting.

2nd. We do not find that constant vomiting, as in sea-sickness, however long it lasts, produces jaundice. On the contrary, *vomiting* is *often* used as a cure for jaundice, as it produces a greater flow of bile from the ducts.

The opinion of Broussais seems most correct, that the yellow colour depends solely on the *violent irritation of the duodenum*, which is propagated to the secreting organ, the liver.

This is nearest the truth, but still does not seem quite satisfactory. If the *irritation* or inflammation of the duodenum was propagated to the liver, we must expect to find that organ inflamed. *However, no such thing occurs in either case.*

Now our *dissections* have, I think, thrown a new light on the subject, and shown the true nature of the obstruction to the flow of the bile which exists in this complaint. In *almost all* the cases of fever with jaundice which have proved fatal, we have found one or more *intus-susceptions of the small intestines*, without any inflammation of the invaginated part (serous membrane). Now let us consider what aid we receive from the finding of these intus-susceptions, towards explaining the origin of the jaundice. But, first, what is the origin of spasm? Inflammation of the mucous membrane of duodenum, and small intestines, and stomach. In dysentery we find evident spasm of large intestines from inflammation, tenesmus, &c. Well, then, having rendered it probable that spasm exists, depending on inflammation, how does this bear on jaundice? We have all heard of spasm of the gall-ducts causing jaundice, and best treated by opium, baths, &c.

We must suppose spasm in the duodenum capable of being propagated to the ducts, or of directly shutting the duct.

This spasm, constantly occurring, produces, every time it takes place, a constriction of the duct, while the quantity of bile is not diminished, the consequence of which is jaundice.

Having thus proved a remarkable coincidence between these diseases, if not their absolute identity, let us see how their treatment agrees. We have found by experience that the only treatment which will serve patients in these cases, is that which has been adopted in yellow fever by the most enlightened and experienced physicians—depletion by lancet and leeches, and large doses of calomel, blue pill, hyoscyamus, &c.

At the time these remarks were penned, I, in common with others, believed that all efferent ducts possessed a vital contractility, because we had observed many phenomena which could only be explained on this supposition. Since then, physiologists have applied themselves to the solution of this question, and it is now generally admitted that these ducts do possess the power of contraction, for which they are indebted to a muscular coat. I shall here quote from the highest authority we possess:—

"The *efferent ducts* of glands are lined by a mucous membrane, which has on its exterior an extremely thin layer of muscular substance. The existence of muscular fibres cannot, it is true, be demonstrated anatomically, but physiological observations place it beyond dispute. The efferent ducts of most glands have the power of contracting when irritated. The contractile power of the ductus choledochus in birds was known to Rudolphi. By irritating mechanically, or by galvanism, the ductus choledochus of a bird just dead, I have frequently produced a very strong contraction of it, which continued some minutes, after which the duct resumed its previous state. I have often excited strong local contraction of the ureters likewise, both in birds and in rabbits, by the application of a powerful galvanic stimulus. Tiedemann also has seen motions in the vas deferens of a horse ensue on the application of a stimulus. It appears, indeed, that periodic vermicular motions are performed by the efferent ducts, at least by the ductus choledochus in birds; for once, in a bird just killed, I observed contractions of the duct to occur regularly in pauses of several minutes; the tube dilating again in the intervals. It was here remarkable, that the contractions took place in an ascending direction, namely, from the intestine towards the liver; which seems to throw some light on the mode in which the bile at certain times, instead of being expelled into the intestines, is retained and driven into the diverticulum of the duct, namely, the gall-bladder; the complete closure of the mouth of the duct contributing, perhaps, to this effect.

"The discharge of the bile from the gall-bladder during digestion results probably from the mere pressure of the surrounding parts, and the action of the abdominal muscles, while the mouth of the duct is open: for I doubt if the bladder is contractile: I could produce no contraction of it in mammalia and birds, even with the most powerful stimulus of a galvanic battery; and in this respect it differs from the other diverticula of efferent ducts, namely, the urinary bladder, and the vesiculæ seminales, which it resembles in all its characters.

"Dr. G. H. Meyer however states that, by means of a galvanic battery of fifty pairs of plates, he has caused the gall-bladder of an ox to contract so as to diminish its capacity one-fourth.

"How far the contractility of the ducts may contribute to the frequently sudden expulsion of the saliva and tears, is a question which I mention merely as requiring further investigation. I may, in conclusion, remark that, since the contractility of the ducts of glands is proved experimentally, *the spasm of these parts, spoken of by physicians, ceases to be a mere hypothesis.*"[*]

[*] Müller's *Physiology*, translated by Baly, 2nd edition, p. 520.

It may be well now briefly to consider how far the Dublin fever of 1826—7 agreed with that since observed at Gibraltar, by Louis.

The prominent symptoms in the yellow fever of Gibraltar were, flushing of the face, headache, suffusion and pain in the eyes, pains in the limbs, thirst, and loss of appetite; *it was rare that the patient complained of any pain in the epigastrium at first, but this generally came on fifteen or sixteen hours from the commencement of the disease, and was then inconsiderable, and very few patients complained of severe or acute pain.* The abdomen preserved its form, was supple and indolent, except in the epigastric region. The yellow appearance of the skin did not come on *till late* in the disease, *and was seldom very intense*, and it was about the same period *that the vomiting and dejections* assumed their peculiar character; the dejections were black or bluish, and the matter vomited, from being of a yellow colour, became black or brown. You will at once perceive that the symptoms which attended the cases of yellow fever we witnessed in 1826, indicated a more intense disease of the abdominal viscera—in *all* there was tenderness over the epigastrium, which in some was excessive—black vomiting did not occur in all, but even in the yellow fever of tropical countries it is not constant; but the symptom which presented the greatest difference in the two epidemics was the yellowness of the skin, which in the fever of Gibraltar came on towards the latter period of the disease, and *was seldom very intense*, but in our fever it came on suddenly, immediately after the tenderness of the epigastrium was complained of, and was in all very intense. This shows that whatever lesion produced the yellowness in the Gibraltar fever was either different in kind or in degree from that which caused it in ours, and I think we cannot doubt but that it was here produced by spasm of the ducts leading from the liver and gall-bladder.

It is well known to pathologists since the time of Broussais, that jaundice is as frequently produced by duodenitis as hepatitis, if not more so—but I do not think that the explanation he gives is applicable to our cases. He concludes that when the mucous surface of the duodenum is thrown into a state of excitement, we may have a consequent affection of the liver, for the duodenum bears the same relation to the liver as the mouth does to the parotid gland; and we know that an irritation of the orifice of the ducts leading from this and other salivary glands is immediately followed by an increased flow of their secretions. But our dissections have shown that the small intestines were affected not only by inflammation, but were acted upon by violent spasms, producing invaginations of different portions of the canal; and there can be no doubt that the ducts (possessing such considerable vital contractility) participated in these spasms, and thus prevented the flow of bile into the duodenum as effectually as if they were tied by a ligature, or their canals obstructed by calculi, and this explanation obtained great support from the fact, that the jaundice came on *suddenly* in most of the cases, *and was always preceded, or accompanied, by violent and convulsive contractions* of the abdominal muscles and intestines.

There is another point to which I am anxious to direct attention. The yellow fever I have now described occurred in the course of an epidemic of continued fever, whose type was a severe and very fatal form of gastro-duodenitis. Does not this circumstance tend to confirm the opinion of Tommasini and others, that yellow fever is but a more severe form of the gastric variety of typhus? The appearance of the liver described by Louis has not been noticed by other pathologists, and cannot be considered the essential *anatomical character* of yellow fever generally; for we read that Rush, Lawrence, Jackson, and Ashbel Smith, the learned writers on the yellow fever of America, seldom found the jaundice connected with liver disease, but that in all cases there was inflammation of the digestive surface: and in the late epidemic of yellow fever which prevailed in Martinique from 1839 to 1841, M. Rufz states that he observed the yellow appearance of the liver, described by Louis, only in two instances, and that this organ, like the rest of the solid viscera, was very often gorged with blood.

Dr. Nott says:—"Of eight cases dissected during the epidemic of 1843 in Mobile, the livers in two only corresponded with the description of M. Louis. They were pale, and when torn resembled very closely gingerbread or new leather; and the six others were of a dark blue or dark chocolate, presenting different shades of colour, and, instead of being dry, they were excessively engorged with blood. The latter cases correspond with the description given by Dr. Hulse of the cases dissected in the Marine Hospital at Pensacola in 1841. Of the eight dissections in 1844, the livers of four corresponded with the description of Louis, two were of a dark olive, and two were perfectly natural. Taking the whole sixteen cases collectively, six were some shade of yellow, dry, and friable; two olive; two normal; and six darker than natural, and much engorged." Dr. Nott also thinks that Louis has fallen into another error in supposing this liver to be peculiar to yellow fever, for he has repeatedly met with it in individuals dying of other diseases, and who never had yellow fever.

In the Martinique epidemic the principal pathological appearances were the following:—"The stomach contained matter of a black colour, generally in great quantity, and the mucous membrane was coloured by this substance; but when the contents were removed, and the mucous membrane washed, he found that it presented a beautiful rose-coloured hue, extending all over its surface, and not produced by distinct vascular arborizations. In the midst of this redness, he observed several round and distinct spots, produced by the effusion of small quantities of dark-coloured blood, having all the appearance of spots of *purpura hemorrhagica*. The mucous membrane was neither thickened nor softened, but was evidently much more easily detached than in the natural condition. The small intestines contained a greyish white matter, particularly the jejunum; the mucous membrane presented precisely the same appearance as the stomach, but the hemorrhagic spots were more numerous and much larger. The glands of Brunner were in a few cases enlarged to the size of millet seeds, but in no instance were the glands of Peyer in the least altered."

During the prevalence of the yellow fever in 1826—7, a captain of a West Indian vessel was admitted into hospital with the disease. He had yellow fever in Jamaica, and stated positively that he was, when under our care, affected in precisely the same manner as he had been in Jamaica; and he also remarked that the other patients seemed to labour under exactly the same kind of fever as he had then witnessed.

The correctness of the views here propounded as to the identity of the cases of yellow fever occurring in the Irish epidemic of 1826—7, with the yellow fever of warmer climates, has been singularly and remarkably proved by the Scotch epidemic of 1843—4, in which cases of yellow fever were very frequent. Dr. Arrott, physician to the Dundee Infirmary, says:—"The similarity of the symptoms during life, and of the morbid appearances observed after death, so nearly agree with the description of the yellow fever of the West Indies, and with the minute accounts of the Gibraltar epidemic of 1828, given by Louis, as to leave little doubt on my mind, that the only difference between these diseases and the Dundee epidemic, if difference there be, is a difference in degree and not in kind." And Dr. Cormack, in his essay on this fever, remarks, "That in all stages of this disease, it is the affection of the stomach that affords the most distinguishing and important symptoms. As it advances, an unconquerable irritability of this organ comes on. Whatever is swallowed, whether solid or fluid, of whatever quantity or quality, is immediately rejected by vomiting. An almost incessant retching takes place even without any extraneous irritation, which commonly, on the third day, ends in what is called the black vomit, the most hopeless of all the symptoms attending it."

In June, 1846, during the very hot weather which then prevailed, I saw two fatal cases of yellow fever. The first was a very athletic gentleman, 24 years of age, who overheated himself by violent exercise, after having travelled without resting during the night. Being exposed to a thorough air he was chilled, and having spent a restless night, on the following day was attacked with intense fever, nausea, vomiting, thirst, pain in the head, &c.; he became yellow on the third day, and died on the fifth, without black vomit.

The second I saw with Mr. O'Reilly of Sackville Street. It was the case of a captain of one of the Liverpool mail packets, who got a chill in the railway carriage coming from Kingstown, when in a perspiration, and was attacked next day with violent fever; gastric and cerebral symptoms predominated, and about the sixth day he became tympanitic, had black vomit, and died on the eighth day.

I also saw a third case in the summer of 1847, in a young girl, aged about 14, which terminated fatally; the only morbid appearance to be observed on post-mortem examination which could have any connexion with the disease, was that the gall-bladder was completely empty of bile.

LECTURE XXII.

SCARLATINA.—EPIDEMIC OF 1801—2—3—4.—EPIDEMIC OF 1834.

It is my intention to-day to make some observations on the scarlet fever which now prevails as a destructive epidemic, in Dublin and many other parts of Ireland.* The history of such epidemics is very interesting, and tends to shed much light, not only upon the changes which diseases undergo, but upon the fluctuations of medical opinions and treatment.

In the year 1801, in the months of September, October, November, and December, scarlet fever committed great ravages in Dublin, and continued its destructive progress during the spring of 1802. It ceased in summer, but returned at intervals during the years 1803—4, when the disease changed its character; and although scarlatina epidemics occurred very frequently during the next twenty-seven years, yet it was always in the simple or mild form, so that I have known an instance where not a single death occurred among eighty boys attacked in a public institution. The epidemic of 1801—2—3—4, on the contrary, was extremely fatal, sometimes terminating in death so early as the second day, as appears by the notes of Dr. Percival, kindly communicated to me. It thinned many families in the middle and upper classes of society, and even left not a few parents childless. Its character seems to have answered to the definition of the *scarlatina maligna* of authors, for a description of which I beg leave to refer you to the *Cyclopædia of Practical Medicine*, where you will find an article on the subject by Dr. Tweedie. In making this reference, however, I do not wish to be understood as expressing my unqualified approbation of the article in question, for I must in candour confess that it falls far short of what we might have expected from a physician of Dr. Tweedie's learning and experience.

The long continuance of the period during which the character of scarlet fever was either so mild as to require little care, or so purely inflammatory as to yield readily to the judicious employment of an antiphlogistic treatment, led many to believe that the fatality of the former epidemic was chiefly, if not altogether, owing to the erroneous method of cure then resorted to by the physicians of Dublin, who counted among their numbers not a few disciples of the Brunonian school; indeed, this opinion was so prevalent, that all those whose medical education commenced at a much later period, were taught to believe that the diminished mortality of scarlet fever was entirely attributable to the cooling regimen, and to the timely use of the lancet and aperients, remedies interdicted by our predecessors. This was taught in the schools, and scarlet fever was every day quoted as exhibiting one of the most triumphant examples of the efficiency of the new doctrines. Thus I myself learned—this I taught; how erroneously will appear from the sequel. It was argued, that had the cases which proved fatal in 1801—2 been treated by copious depletion in their very commencement, the fatal debility would never have set in, for we all regarded this debility as a mere consequence of previous excessive reaction. The experience derived from the present epidemic has completely refuted this reasoning, and has proved that, in spite of our boasted improvements, we have not been more successful in 1834—5, than were our predecessors in 1801—2.

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* This lecture was delivered during the session of 1834—5.

Before I detail more particularly the symptoms that accompany the present epidemic, I wish to enter a little at large into the subject of the changes and variations which the same disease is observed to undergo at different periods of time. This is a topic which occupied some of the master minds of antiquity, and upon which the greatest of modern physicians, the illustrious Sydenham, bestowed considerable labour. It has been too much neglected of late, and consequently I consider it my duty to call your attention to it, and I cannot do this better or more forcibly than by communicating to you a literal translation which I have made from the German of my friend Dr. Autenrieth's observations on this subject. The task of translation is always not only difficult but irksome; but if, as in the present instance, I can by this means convey to you valuable information not before presented to my class, or to the public in England, I never decline the labour. What I am now about to read is indeed most important, and well deserves the deep attention of every practical physician.

"The third cause, connected with time and capable of modifying diseases, is of infinite importance, both in a theoretical and practical point of view, but has seldom attracted much attention. Its existence is attested by its effects alone, for its nature remains unknown. I allude to the *constitutio morborum stationaria*, first noticed by Sydenham, but since his time nearly forgotten, or else confounded with the permanent influence of the seasons, or the accidental atmospherical changes spoken of above. All diseases, contagious or non-contagious, acute and chronic (the latter, however, seldom, except when attended with some degree of general excitement), have been observed to preserve a certain *constitution or general character*, which continues for a number of years in succession, with occasional interruptions, until it is displaced by another constitution of a different character. Thus, during one period diseases are remarkable for being frequently accompanied by a sensation of extreme weariness, sudden sinking of the strength and vital powers, unpreceded by any evident marks of excitement, and attended by a disposition to pass into true typhus. During another period, the tongue is in general loaded with a thick white or yellowish coat; and many other symptoms of derangement in the digestive organs, such as a bitter taste, costiveness, or diarrhœa, are constantly observed.

"During a third period, diseases are characterized by a remarkable degree of vascular excitement, an evident tendency to local determinations, a frequent formation of morbid productions; in a word, by all the symptoms of inflammation.

"It is not known whether the transition from one of these periodic constitutions to another takes place suddenly or gradually; but the latter supposition appears more probable except when the transition is accompanied by unusually great atmospheric changes. The erysipelatous affection, which, both in England and Germany, succeeded the gastric and accompanied the first appearance of the inflammatory period, seems to have been an example of the gradual transition. Accurate observations are still wanting to determine whether this periodic constitution is confined to certain parts of the world or extends over the whole, and whether its different species follow each

This book is the prop
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other in a regular order of succession. If their order of succession should at any time be determined, it will enable the physician to foretell the character and most appropriate treatment of future diseases. The above questions cannot be answered without very great labour spent in the investigation of the history of diseases in all ages and all countries, and are therefore foreign to the present work.

"The general indications of course vary with the nature of the prevailing constitution; and, consequently, during one period stimulating remedies, during another alvine evacuations, and during a third venesection and the antiphlogistic plan, will constitute the most effectual treatment.

"This very circumstance has caused much confusion in medical opinions, and has occasioned the reputation and the downfall of many an infallible system, each of which is in its turn consigned to oblivion, and perhaps again revived as a novelty at some future period. The English boast much of the astonishing improvements in science, and deride the ignorance of their predecessors, regardless of the old proverb,—'Everything has its day.' Whenever, therefore, the periodic constitution undergoes an alteration, they either obstinately uphold their usual plan of treatment, to the manifest injury of their patients, or else blindly embrace some system, to them new, but which really rests upon ancient and established principles. In general, they do not fail to make use of so much exaggeration in support of their opinions, and thus succeed in misleading so many, that none but very well-informed physicians can distinguish the fallacy of their arguments.

"The medical history of Great Britain affords many striking proofs of the truth of these assertions, and is replete with examples of the singular obstinacy with which the English cling to opinions once formed, a circumstance which has materially contributed to obstruct their attaining to general views and impartial conclusions. Even to this day, a warm contest is carried on (less, however, in books than in the debates of learned societies) between the senior and the junior parts of the profession, the former still inclining to Brunonianism, while the latter attribute nearly all diseases to inflammation. Both, indeed, appeal to experience to prove the justice of their principles, and seem entirely to forget that while the propriety of their practice, as applied to particular cases, remains unimpeached, the very nature of the diseases themselves may have been changed. A summary review of the character assumed by diseases during the last twenty years, both in England and other countries, will perhaps afford a solution of this question. About the end of the last and during the three or four first years of the present century, the proportion of nervous fevers to other diseases was as one to eighteen in Plymouth (Woolcombe), as one to sixteen in London (Willan), as one to ten in Newcastle (Clarke), and in Liverpool, one to five (Curry). Nor was this scourge of mankind less severely felt upon the Continent, where typhus, and diseases closely allied to it, committed extensive devastations, particularly during the epidemics of Erlangen, Jena, Kiel, Ratisbon, and Vienna. Cadiz and Seville were at the same period depopulated by the yellow fever, and Europe in general suffered much from repeated visitations of influenza. An inclination to a sudden sinking of the vital power, unpreceded by violent reaction, and unaccompanied by any marked symptoms of a gastric or inflammatory nature, constituted at that period the characteristic form of acute diseases, which were always preceded and attended with an unaccountable degree of debility. Stimulating and tonic medicines obtained, therefore, much celebrity, and every physician who practised during that period attests the injurious or even fatal effects which were produced by the use of venesection, and other depletory remedies. What is still more remarkable, an epidemic typhoid pneumonia prevailed in many parts of Germany during the years 1800—1—2, in which the speedy production of an inflammatory state, by means of bark and ether, was the only method which afforded a chance of recovery. These facts must impress every impartial mind with the conviction, that the constitution of diseases has undergone much alteration since that period, and explain why physicians did not then employ copious venesection, but were obliged to content themselves ordinarily with cold affusions, acids, and mercury.

"The reign of typhus appears to have ceased with the influenza of 1804, when a new constitution began, at first more remarkable for the disappearance of nervous fevers and other contagious diseases, than for any peculiar character of its own. Catarrhal and rheumatic complaints, partly attributable to the weather, prevailed for some time, and fevers of an intermitting type became more frequent, forming an evident transition from the purely typhus constitution to that of the vascular excitement of the following years. Some remnant of the typhus constitution was indeed still perceptible in the pectoral complaints which prevailed in London during the winter of 1804—5, and were attended with remarkable debility, requiring the greatest prudence in the use of the lancet. Venesection was indeed often entirely contra-indicated, and Bateman states it sometimes even proved fatal. The constitution, however, soon developed itself more decidedly, became more universally diffused, and obliged physicians to relinquish their former plan of treatment and adopt other measures. Derangement of the alimentary canal became its prominent feature in the summer and autumn of 1804, and diarrhœa, terminating in dysentery, was often met with.

"This constitution suffered indeed a check from the cold of 1805, but it increased again during the following years, and afterwards became still more prevalent, manifesting itself by headache, a bitter taste in the mouth, a loaded yellow tongue, irregularity of the bowels, nausea, and anorexia. The utility of purgatives now became so obvious, that Hamilton's doctrines soon obtained as much celebrity as had been before enjoyed by the stimulating system. The nervous fever at Nottingham in 1807, the dysentery at London in 1808, the scarlatina at Edinburgh in 1805, and the measles at the same place in 1808, all required the purgative plan of treatment, and calomel became the favourite cathartic. The advantage then derived from the use of purgative medicines is abundantly testified by the writers of that period. This gastric constitution appeared also on the Continent, but its progress was less rapid there than in England, where the inhabitants live in a manner calculated to augment or even to produce a tendency to gastric diseases. There were likewise other circumstances which impeded the formation of this constitution on the Continent. Thus in Germany, the purely nervous constitution had scarcely yielded to catarrhal and rheumatic affections, when it was again revived in that unhappy country by the political occurrences of 1805—6—7. Typhus seldom, however, assumed the character of exquisite, for the rheumatic and catarrhal affections with which it was mixed partook somewhat of a gastric nature, as was proved by the great benefit derived from the exhibition of emetics and calomel. This appears in accordance with the fact, that the gastric constitution was more fully developed wherever the ravages of war had not extended, although it still required less attention in the treatment than the rheumatic symptoms, then likewise prevalent. Thus the agues which were common at Tubingen about the end of 1806 commenced in general with pain in the belly, vomiting, and irregularity of the bowels; a yellow furred tongue, headache, and tumors of the parotids were of frequent occurrence, and in general gastric symptoms were by no means rare. These symptoms gradually gained ground, and the reputation of ipecacuanha and cathartics increased in the same proportion. At Ratisbon the *constitution* was remarkably gastric in the autumn of 1809, and a nervous fever prevailed at Weimar in 1809—10, which was accompanied by bitter taste in the mouth, diarrhœa, nausea, and vertigo. Active catharsis was injurious in this epidemic, but much benefit resulted from the exhibition of castor oil. The advantage derived about the same time in Berlin from the treatment of fevers by emetics and cooling purgatives, proved that they were there also complicated with gastric derangement.

"The gastric constitution had scarcely established itself, or become pretty generally diffused, when a new character, viz., the inflammatory, appeared upon the stage, and has ever since continued, sometimes combining itself with the gastric to form diseases of a mixed character, such as erysipelas, and sometimes, when favoured by the seasons or local circumstances, raising itself to the rank of the chief performer. With its appearance, venesection, which had previously fallen into disrepute, became once more a favourite remedy, and in the course of a few years was pushed so far, particularly in Great Britain, that Sangrado's maxim, 'C'est une erreur de penser que le sang soit necessaire à la conservation de la vie, on ne peut trop saigner un malade,' seems to have been the general rule of practice. The same inflammatory constitution became also general in Germany, but there it neither attained such a height, nor required such active treatment as in Great Britain, where many circumstances favoured its more perfect development; with us it generally yielded to the use of acids, cold applications, and mercury, but in England it called for copious blood-letting. Even in 1810, diseases had become more inflammatory at Tubingen than they had been previously; but the change was still more perceptible in 1813, when the antiphlogistic treatment required the aid of small venesections, and nervous fevers were accompanied both by inflammation and derangement of the digestive organs. Erysipelatous affections were also frequent, and in many cases were of a marked inflammatory character. Erysipelas and true inflammatory fever, requiring the use of the lancet, were common at Ratisbon in 1811. Parrot exhibited acids, especially the acetous, with great success in the epidemic nervous fever which raged at Dorpat in 1812, and a diarrhœa of a bilious inflammatory nature prevailed at Königsberg during the same year. This important change in the *constitution* became very evident in the nervous fever at Berlin in 1813, as well as in the formidable epidemic described by Hufeland, which ensued after the war, and raged in the north of Germany during that and the preceding year. Although but a few years before the strongest stimulants had been necessary to obviate the paralysis which supervened even in the beginning of the disease, yet an opposite practice was now required, and antiphlogistic remedies were alone found capable of preventing the vascular excitement from terminating in inflammation of either the head or chest. In short, the inflammatory *constitution* has been prevalent in Germany ever since the years 1810—11, sometimes in its pure and marked form, and sometimes complicated with gastric and rheumatic symptoms.

"This *constitution* became general at the very same period in Great Britain. Dr. Clutterbuck, of London, had indeed ascribed the origin of fever to inflammation of the brain, so early as 1807, and about the same time Dr. Steiglitz, of Hanover, had recommended the antiphlogistic treatment of scarlet fever, in preference to the stimulating plan then in vogue. But as the inflammatory was then still subordinate to the rheumatic and gastric constitutions, their opinions did not gain many converts. But the inflammatory constitution had increased so much in the autumn of 1809, and the winter of 1810, that even Bateman was obliged to prescribe venesection in fevers—a practice quite at variance with his former views. Erysipelatous inflammation became common in London, Aberdeen, and Leeds, and numerous cases of puerperal fever occurred in the latter towns, which, according to Gordon and Hey, never terminated favourably, except when bleeding and purgatives were employed with freedom. But it was not until 1813, when the inflammatory constitution had fully developed itself, and the bad consequences arising from violent determination of blood to the head in nervous fever could not be averted except by decisive measures, that venesection came into general use in Great Britain, in consequence of a publication by Dr. Mills, who had prescribed it with much success since 1810. In the same year that truly estimable physician, Dr. Thompson, published his admirable work upon inflammation. Blackall recommended blood-letting in several species of dropsy, and Armstrong employed the same remedy, combined with large doses of calomel, in the inflammatory puerperal fever which was prevalent in Sunderland. Venesection became from this time as great a favourite as ever in England, not, however, to the exclusion of purgatives, which were indicated by the derangement of the stomach and bowels that accompanied the inflammatory constitution. Both these remedies were found extremely beneficial in the nervous fever which was epidemic in Ireland in 1813—14 ; its inflammatory character being clearly evinced by a hard and full pulse during its first stage, and a violent determination of blood to the head, by which the headache and raving are increased, while its gastric type was not less strongly marked by tenderness of the epigastrium, costiveness, or else frequent and unnatural alvine discharges, together with a loaded tongue and bilious vomiting. The latter symptoms were, in Dr. Grattan's opinion, of such importance, that he gave a decided preference to the purgative plan. The fever, which had previously been confined to Ireland, became generally diffused over the rest of Great Britain after the famine of 1816, and continued without intermission for four years. Its inflammatory character being peculiarly favoured, both in England and Scotland, by the habits of the inhabitants and the situation of these countries, venesection attained an unexampled degree of celebrity, notwithstanding the representations of the Irish physicians, who used that remedy with more moderation. It was soon believed that there is, literally speaking, no disease whatever in which the lancet ought not to be used, and, as the human mind is ever prone to extremes, it was soon generally considered, both in England and Scotland, to be a well-founded pathological inference, " there is proof that the congestion of internal organs was not caused by any retrocession of the eruption. In truth, as will appear hereafter, the worst cases had the most general and most intense cutaneous efflorescence. When this tendency to the head took place in so violent a manner at the very outset, the patient was seldom saved; sometimes, however, very active measures of depletion, general and local, relieved the brain, and the case then went on favourably. This happened in a young gentleman residing in Upper Baggot Street, to whom I was called by the late Mr. Nugent, of Merrion Row. When the scarlet fever attacked a person subject to epileptic fits, the tendency to the head was increased by the epileptic habits, and fits of convulsions at once supervened. Thus, in the case of a gentleman, aged 22, who had been for several months treated by Mr. Colles and myself for epilepsy, the fits commenced on the second day of scarlatina, and continued with frightful violence until the fifth day, when they proved fatal. In a young lady residing near Blackrock, to whom I was called by Dr. Wilson, precisely the same thing occurred. She had been subject to epilepsy for many years, and when the scarlet fever commenced, she was at once seized with frequently recurring fits, which, in spite of the most active measures, ended in fatal coma on the fifth day.

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[illegible]

[illegible] for the present been obliged, by the pressure of [illegible] to postpone a more accurate examination of [illegible], and a more severe scrutiny of the facts which just [illegible] my memory; but I conclude with remarking, that the [illegible] epidemic of influenza, which in 1833 visited [illegible] of Europe, including the British Isles, was not only [illegible], both for the violence of the feverish symptoms and the local congestions of the chest and heart which accompanied its attack, but likewise for the unexpected relation which [illegible] to bear to all measures of active depletion. I appeal to the profession for their testimony on this matter—I ask [illegible] preconceived opinions as to the *a priori* in- [illegible] venesection, leeching, and purging, were not found [illegible] by the effects of these remedies in that epidemic. The sudden manner in which the disease came on, the heat of skin, acceleration of the pulse, and the intolerable violence of the headache—together with the oppression of the chest, cough, and wheezing—all encouraged us to the employment of the most active modes of depletion, and yet the result was but little answerable to our expectations, for these means were found to induce an awful prostration of strength, with little or no alleviation of the symptoms. In some who were thus treated, recovery was protracted and doubtful, and the strength was not restored for several months. Indeed, nothing was more curious than the length of time which was necessary for some persons, in order to recruit their strength after an attack of influenza, although that attack had not continued more than a few days, and had been judiciously treated, without blood-letting or unnecessarily debilitating remedies. I have known some who lapsed into a cachectic state of long-continued debility from which they never recovered; for, while thus reduced, they fell victims to the first acute complaint which seized them. The influenza above referred to fully confirmed the opinion I had long entertained, that in acute diseases debility and exhaustion of the vital power are by no means in every case either caused by, or proportioned to, a state of previous excitement. This opinion received further support from the symptoms and phenomena exhibited by the Asiatic cholera, in which the stage of debility and collapse commenced, and too often closed the scene; and has been still more powerfully corroborated by the epidemic of influenza of 1837 and 1847, as also by the Irish epidemic fever of 1846—47. Why do I dwell upon these occurrences, and why have I so frequently referred to the opinion above expressed? Simply because the prevalence of the contrary opinion laid the foundation for the injudicious and exclusive application of the lancet, and of the antiphlogistic method generally in Great Britain, and was, consequently, the cause of working excessive mischief.

I have already mentioned that the disease called scarlet fever assumed a very benign type in Dublin soon after the year 1804, and continued to be seldom attended with danger until the year 1831, when we began to perceive a notable alteration in its character, and remarked that the usual undisguised and inflammatory nature of the attack was replaced by a concealed and insidious form of fever, attended with great debility. We now began occasionally to hear of cases which proved unexpectedly fatal, and of families in which several children were carried off; still it was not till the year 1834 that the disease spread far and wide, assuming the form of a destructive epidemic. The nature of the disease did not appear in the least connected with the situation or aspect of the patient's dwelling, for we observed it equally malignant in Rathmines as in Dublin; on the most elevated habitations on mountains, as in the valley of the Liffey. It raged with similar violence at Kingstown, and the neighbourhood of Killiney and Bray. The state of the weather seemed to exercise no influence either upon its diffusion or its symptoms, which continued to exhibit equal virulence, no matter whether it was wet or dry, warm or cold, calm or stormy. The contagion seemed to act as a more deadly poison on the individuals of some families than upon those of others, and consequently, when one member of a family died, there was always much reason to fear for the others when attacked. At first I thought that its greater severity in such cases could be traced to strumous habit, but subsequent experience did not confirm this suspicion, for the most scrofulous family I ever saw went through the disease without a death, whereas in some others the mortality was great, although not a single indication of a strumous diathesis could be detected. Many parents lost three of their children, some four, and in one instance which came to my knowledge, five fine children were carried off. As usual in such epidemics, the degree of intensity with which different persons were attacked varied exceedingly, some exhibiting the mildest form of scarlatina simplex, which required no treatment, and scarcely confinement to the room, while the majority were severely affected. When the disease was violent, it assumed one or other of the following forms:—

First.—It at once produced not merely fever with sore throat and headache, but such violent congestion of the brain, and determination to the head, as occasioned convulsions and apoplectic coma on the first or second day. This happened to a young woman of robust habit in Werburgh Street, to whom I was called by Dr. Brereton. She was attacked with convulsions on the second day, and died comatose on the third. In her the scarlet eruption was extremely vivid and general, a fact I notice as a proof that the congestion of internal organs was not caused by any retrocession of the eruption. In truth, as will appear hereafter, the worst cases had the most general and most intense cutaneous efflorescence. When this tendency to the head took place in so violent a manner at the very outset, the patient was seldom saved; sometimes, however, very active measures of depletion, general and local, relieved the brain, and the case then went on favourably. This happened in a young gentleman residing in Upper Baggot Street, to whom I was called by the late Mr. Nugent, of Merrion Row. When the scarlet fever attacked a person subject to epileptic fits, the tendency to the head was increased by the epileptic habits, and fits of convulsions at once supervened. Thus, in the case of a gentleman, aged 22, who had been for several months treated by Mr. Colles and myself for epilepsy, the fits commenced on the second day of scarlatina, and continued with frightful violence until the fifth day, when they proved fatal. In a young lady residing near Blackrock, to whom I was called by Dr. Wilson, precisely the same thing occurred. She had been subject to epilepsy for many years, and when the scarlet fever commenced, she was at once seized with frequently recurring fits, which, in spite of the most active measures, ended in fatal coma on the fifth day.

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The second form of scarlatina was likewise remarkable for the violent excitement manifested from the very beginning in the circulating system, and in the production of animal heat. The pulse at once rose to above 100, it was seldom less than 120, and in many cases, particularly in young people, it ranged from 140 to 150. I have never in any other disease witnessed so many cases of excessively rapid pulse. In general the pulse in this form was regular, but in two cases it became irregular. One was that of a gentleman living in Upper Mount Street, whom I attended with Sir Henry Marsh; his pulse became intermitting and irregular on the third day, and continued to be thus affected more or less for about a week. This gentleman was attacked with subsultus, delirium, jactitation, and various nervous symptoms, at a very early period, and complained constantly of his throat and head. The former was violently inflamed, and his skin was covered with a bright red eruption. On the ninth day he was seized with convulsive fits of great violence, and which returned very frequently during the night; his case appeared utterly hopeless, and yet he perfectly recovered. In a young lady who was attended by Dr. Nolan, great irregularity and intermission of the pulse commenced about the eighth day, and continued during the state of danger; she also recovered. Of course irregularity of the pulse was in many not so much a symptom of disease as of approaching death, but then the state of the patient could not be mistaken, judging from all the other circumstances of the case. The acceleration of the pulse abated in all when an evident improvement in the general condition took place, but in few did the pulse become quite natural for many days after the favourable change, and in none did it fall to its usual standard in the course of twelve or twenty-four hours, as it not unfrequently does after the crisis of continued fevers; in fact, the scarlatina never ended with a well-defined crisis.

As to the temperature of the body, I have already observed that in the cases I am now describing it was from the first considerable, and continued elevated until a very short period before death. Both the pulse and heat of skin, however, were very easily reduced in energy by the use of the lancet, or by the repeated application of leeches, and it was not uncommon to observe that even the judicious use of these means induced a general coldness of surface, very great sinking of the strength, and a faltering state of the pulse. This was remarkably the case in a young lady whom I attended along with Mr. Wilkinson in Blackrock, and also in one of the family for the history of whose cases I am indebted to Dr. Nolan. In both, these effects were very obstinate and alarming, for reaction was not restored until after the lapse of more than twelve hours, but both finally recovered. The pulse was sharp but not strong, and resembled the pulse of great irritation rather than that of true inflammation.

The most distressing symptom at the commencement of this form of scarlatina was the sore throat; the fauces were violently inflamed, and deglutition consequently much impaired, while a general soreness was felt in the back of the head and neck; urgent headache was complained of by all, and from the second day the eyes became suffused; great restlessness, anxiety, jactitation, moaning and interrupted raving soon made their appearance, and in many sleep was banished or utterly broken by startings and delirium before three or four days had elapsed. The eruption had now arrived at its height, which it did with great rapidity, dating from the first moment of its appearance, so that the skin, everywhere covered with a scarlet eruption, resembled in appearance the hue of a boiled lobster.

In these violent cases the efflorescence was perfectly continuous, and never broken into spots or patches; the skin appeared as if evenly dyed with one uniform colour; the surface of the tongue was likewise much affected with the same exanthematous redness, and soon became foul, and afterwards dry and parched. The sudden drying of the tongue on the fifth or sixth day indicated in this form a rapid aggravation of the disease, and death in several cases was observed to follow this change in less than twenty-four hours, when it was, as in a young gentleman Mr. Rumley and I attended in French Street, accompanied by a sudden acceleration of the pulse and increase of the jactitation and delirium. In this form the brain and nervous system seemed to be the parts which suffered most, and many became insensible for several hours before death; others had convulsions. When the patient survived the seventh day there was a fair chance of recovery, but many, too many, died on the fourth, fifth, or sixth days.

After I had witnessed a few examples of this form of scarlatina, I consulted with several of my friends and colleagues, and we determined to use the most active measures of depletion in the very first instance that occurred to us. A case was not long wanting. Sir Henry Marsh and I were engaged in prescribing for some children labouring under the epidemic, in a house in Pembroke Street, where our attention was directed to a fine boy, 6 years old, and hitherto perfectly healthy, who was, while we were paying our visit, attacked with the first symptoms of the complaint; we immediately resolved that as soon as the stage of rigor and collapse which preceded the febrile action had passed, to visit him again and act energetically, if circumstances seemed to permit it. Accordingly we came again in the course of a few hours, and found reaction already established, attended with vomiting, purging, and headache. The sore throat, too, was much complained of, and there was great tenderness of the external fauces. We ordered relays of leeches, eight at a time, to the neck, for the purpose of relieving both the throat and brain, and we administered James's powder and calomel internally. On the next day the skin was burning, in spite of a copious loss of blood from the leech-bites, the eruption vivid and already established, the pulse 140, and there had been little or no sleep. Relays of leeches were again ordered, and persevered in until considerable and lasting faintness was produced, and yet no impression seemed to be the result, for the raving became more incessant on the second night, and on the third day suffusion of the eye commenced, and the tongue became parched. Shaving of the head, the most industrious application of cold to the scalp, and various other remedies were in vain applied; the pulse became weaker, the breathing quicker, the strength failed rapidly, raving and delirium gave place to insensibility and subsultus, and the patient died on the fifth day. In this case depletion was applied at once and decidedly, for we blanched and weakened the boy by loss of blood as far as it was possible to venture, and yet the disease was not in the least degree checked, nor the symptoms even mitigated.

A fine boy, 13 years of age, was attacked in the county of Wicklow, where he was placed under the care of a very judicious practitioner, who did not use either venesection or leeches, but relied chiefly on the exhibition of diaphoretics, particularly antimonials. The boy died on the seventh day, having suffered much from delirium, subsultus, want of sleep, &c. His brother, who was one year older, and a very strong boy, was seized with the disease in Dublin, and placed immediately under my care. I had the advantage of Mr. Rumley's assistance, and we determined to prevent the supervention of the cerebral symptoms, if it were possible to do it, by means of antiphlogistic treatment: we failed, and our patient died on the sixth day. In short, this form of the disease, where the pulse, without becoming strong, *became at once extremely rapid*, bore venesection badly, and required great caution even in the application of leeches; the nervous symptoms appeared only accelerated by the system of depletion, although the heat of the skin suggested its employment.

The derangement of the brain and nerves in this form depended on something more than the violence of the circulation, and originated in something altogether different from mere cerebral inflammation or congestion. What that something was I cannot even conjecture: but it was probably the result of an *intense poisoning of the system by the animal miasma of the scarlet fever.* Every tissue of the body seemed, if I may use the expression, equally sick, equally overwhelmed, and it is probable that the capillary circulation in every organ was simultaneously deranged. It was not gangrene of the throat which proved fatal, for in this form it never occurred; it was not inflammation of any internal viscus, for such was not found on post-mortem examination of the fatal cases; but it was a general disease of every part. In many, another state of things, which required to be carefully distinguished from that just described, existed, and the disease was evidently attended with an inflammatory state of the constitution, requiring energetic measures. In such cases the symptoms were severe in the commencement, the throat very sore, the efflorescence, however, not quite so sudden or so perfect, and the pulse not so quick, never excessively rapid, and always strong and distinct. Such bore bleeding and leeching well, and experienced from their use almost immediate alleviation of the sore throat, headache, and restlessness, and were not much weakened by the depletion. It must be confessed that it was often exceedingly difficult to determine, *a priori*, whether the depletory system ought or ought not to be tried. Where doubt existed, my custom was to try moderate leeching, and from its effects I judged of the propriety of persevering.

The disease very frequently occurred in a *third form*, more singular still than the two first, and much more insidious in its commencement. This form was evidently very common in the epidemic scarlet fever described by Withering, as cited by Dr. Tweedie. In this form the disease was ushered in by the usual symptoms of pyrexia, together with sore throat, slight headache, and in due time a very moderate and normal eruption. The symptoms continued moderate; the patients, after the first few days, slept tolerably well during the night, had no raving, and were quiet during the day. About the fourth or fifth day all the febrile symptoms had so far subsided that a most accurate examination could detect nothing urgent, nothing in the slightest degree either alarming or calculated to excite the least anxiety in the patient's condition. His skin became nearly of the natural standard, his thirst diminished, and the pulse was now scarcely accelerated; a calm, nearly complete in fact, seemed to have followed the first onset of the disease; and, on entering the room, the physician might easily be deceived, as I myself was more than once, into the pleasing hope that all danger was past, and that perfect recovery might confidently be anticipated.

This hope was, in truth, founded on such circumstances as we can usually rely on; for who would prognosticate danger, where his little patient, sitting up in bed, and perhaps eating a dry crust with some appetite, had a placid countenance, and had enjoyed a night of tranquil sleep? Regular alvine evacuations, diminution of thirst, sore throat, headache, and fever, together with the normal state of the cutaneous eruption, all conspired to confirm a favourable prognosis; and so matters proceeded, the family dismissing all apprehensions as to the result, and the physician most probably discontinuing his attendance about the seventh day, in the belief that all danger was over, and that his interference was no longer necessary. Matters proceeded thus until the eighth or ninth day, when a certain degree of restlessness was observed to occur, and in the morning a slight return of fever might be noticed. Then it was that a peculiar train of symptoms set in. The nostrils assumed a sore and irritated appearance about the edge of the alæ, and a serous moisture began to flow from their internal cavities. Sore throat was again complained of, the skin became hot, great debility and prostration of strength came on suddenly, a painful tumefaction commenced in the region of the parotids and submaxillary glands. This tumefaction increased rapidly, becoming every day harder, more elevated, diffused, and exceedingly tender, but without much redness. In the course of a few days it surrounded the neck like a collar, and being attended with swelling of the face, the poor little patient's countenance was sadly disfigured. In the meantime the discharge from the nose had increased considerably, and become more viscid and fœtid; the internal membrane lining the nasal passage was affected throughout, its entire surface everywhere inflamed and tumefied, so that a snuffling sound was produced when the patient breathed through his nose: at length the discharge increased to such a degree,

mong the sequelæ which I had occasion to see, diarrhœa
d in two or three instances, chronic bronchitis in one,
asarca in four. The urine was slightly albuminous in
the latter cases before the face and limbs began to swell;
other two it exhibited this character when the disease was
, but I did not see them previously. The treatment of
sarca was antiphlogistic and diuretic, and succeeded in
g three to perfect health. The fourth still remains an
but not from this cause; the apex of the right lung
evidence of tubercular disease.

ave now to mention a peculiar affection of the neck,
have not before seen in connexion with scarlatina, but of
four cases have occurred during my observation of the
c in question.

e 1.—About the beginning of August, 1834, I was
d by my friend, Dr. Davy, to see a young girl, ten
d, in Upper Baggot Street. Her convalescence was
some degree of fever still existing at the end of six
om the commencement of the attack. But her principal
t was severe pain of the right side of the neck, close to
l, and extending as high as the vertex, on the least
of the part. She could not raise her head from the
thout putting a hand at each side for its support, and
ken out of bed, instinctively sought a resting-place for
. The face was awry, its vertical diameter passing
ve downwards, and from right to left. Posteriorly, the
rvical vertebræ were curved, the convexity of the curve
uated a little to the left of the middle line: there was
ble swelling of the soft parts covering the bones.
here was intolerable, and the least attempt to rotate
ccasioned severe pain. Deglutition was now tolerably
here had been considerable difficulty of swallowing
early period of the complaint. There was here
arious state of the articulation of the atlas and
e did not expect to remove the curvature. Perfect
er, enjoined, and the usual remedies employed
st the further progress of the disease. She
her health, and is now lively and well
is permanent.

t, 1834, Mary Inglesby, of Russell

Place, aged 7, was sent to me by Mr. Long, of Summer Hill. She was confined to bed in scarlatina for a fortnight. At the end of this time she was taken out of bed, and then the head was observed to be turned to one side. It was now five weeks altogether from the beginning of the disease, and the parts were still in the same state. The face was awry. She complained of pain in the concavity of the curve and that side of the head, and could not bear the slightest motion or shock. Leeches were prescribed, and calomel given afterwards in doses of a grain three times a day, till the gums were touched. As soon as this effect was produced, the pain subsided, and the head gradually acquired its natural position. Her recovery was complete.

"Case 3.—A younger brother of Mary Inglesby was subsequently under the care of Mr. Long for scarlatina. The same state of the head and neck was detected on the thirteenth day, and treated by Mr. Long on the same plan as that adopted in the former case. The pain disappeared as soon as the mouth was made sore, and the position of the head became natural. He is now in good health.

"Case 4.—I met Mr. Edgar, of Arran Quay, in February, 1835, in the case of a young gentleman about six years old, whose convalescence from scarlatina was tedious, and in whom the difficulty of swallowing persisted after the redness of the fauces was removed. On taking him out of bed it was remarked that he was quite unable to keep the head erect. The symptoms were similar to those of the two last cases, but in a milder degree. A few leeches were applied, and evaporating lotions instantly used to the part, on account of considerable local heat. The leeching was repeated in a day or two, but as the symptoms yielded rapidly, and as he had some tendency to diarrhœa, calomel was not employed. In about a fortnight the natural position of the head and neck was restored.

"I can offer no better explanation of the occurrence of this affection during the progress of scarlatina, than by supposing that the inflammation of the fauces and back of the pharynx was propagated to the adjoining parts. In all these cases there had been marked and prolonged difficulty of deglutition, as a symptom of the disease; and it is to this circumstance I am desirous of calling attention, as affording an index for a careful review of the condition of the spine during the period of convalescence. Should a child be observed to lie more on one side than the other, and evince an unwillingness to be disturbed, it would be an additional reason for suspecting a tendency to this complaint."

LECTURE XXIII.

SCARLATINA.—ITS DIFFUSION AND TYPE IN THE COUNTRY DISTRICTS OF IRELAND.

Since the preceding lecture was delivered, scarlatina has raged every winter and spring with undiminished virulence, resisting, as before, nearly every kind of treatment, until the last two years, 1847 and 1848, when it was much less severe both in extent and character, and consequently much less fatal. A letter which I received in 1842 from Dr. Cumming, of Armagh, stating that scarlatina had rarely been witnessed in that city since he settled there, eleven years previously, and that he had never seen the malignant form of the disease, induced me to forward a circular to the principal medical men in the provinces, to ascertain if the disease prevailed in their respective districts, and if it had assumed the fatal form we had observed in so many instances in Dublin.

I shall now briefly state the principal facts contained in the answers to my queries. Dr. Geoghegan, of Kildare Infirmary, says that, during his residence there for ten years, scarlatina never prevailed as an epidemic, and the sporadic cases he met with were exceedingly mild, until just about the period of the receipt of my letter, when many cases of it occurred at Newbridge, four miles from his residence, on the Dublin road, and, to judge from the number and rapidity of the deaths, of the malignant kind. It was nearly confined to the children of the labouring class, but, not having the dispensary there, he did not see them. One case, however, which he did attend, that of a boy aged five years, then three weeks ill, he describes as follows:—On the *right* temple was a large ecchymosis, about two inches in *diameter*; arterial blood trickling from the nose, mouth, and *ears*; he was greatly emaciated, and quite sensible; had diarrhœa and the *hemorrhage* only from the preceding day; the cervical glands were enlarged, but had not suppurated, nor was there anasarca or dropsy; he died the following morning.

Dr. Astle, of Edenderry, does not remember its occurrence as an epidemic, and the sporadic cases he has seen have been mild. Dr. Woodward, of Kells, has not seen it epidemically, but isolated cases were remarkably fatal, some dying within the first twenty-four hours. Dr. Clifford, of Trim, mentions that it has latterly been prevalent in his district and very fatal. Dr. Clarke, of Rathdrum, states that it has been on the increase for the last three years, but has been very mild. Dr. Lloyd, of Malahide, says, "In reply to your circular relative to the prevalence of scarlatina in my district, I beg to say, the year ending May, 1839, no case occurred; May, 1840, one case in an adult; May, 1841, no case; May, 1842, thirty-two cases are registered, three of which were fatal, one twenty-four hours after the appearance of the eruption; the others were a brother and sister, aged eight and six, scrofulous, and after a period of from twelve to sixteen days they died of diseased brain and abscesses in the throat. Since May, up to this date (August, 20, 1842), six cases have been under my care. The only cases of moment were those mentioned above as fatal, and some of the same family in which the urgent symptoms were extensive ulceration of the fauces—they recovered rapidly. During the past year there were numerous instances of the disease, but so slight that the individuals were under no restraint; so that I was not applied to, save occasionally to treat some of the sequelæ. I may here allude to a curious fact. My district joins on one side that of Baldoyle; on the other, Swords; in both, epidemic diseases have frequently appeared for the last twenty-five years, with virulence, and after a long period commenced in Malahide district, in a mild and subdued form: many of the poor inhabitants are aware of the circumstance."

Dr. Glover, of Philipstown, never saw or heard of a case of scarlatina during the four years he has resided there. Dr. Brunker, of Dundalk, mentions that the disease has only presented itself in one instance within the last five years, and was very mild. Dr. Hudson, of Navan, has not met with the disease often; and during a term of eight years has had only one fatal case; whilst Dr. Byron, of the same town, states that the disease "was very prevalent, and in several localities unusually malignant, during the last two years, up to a period of about two months ago, when it was observed to be on the decline. At present there are very few cases within twelve or fifteen miles of Navan, and these are less virulent, generally speaking, than formerly." From Wexford, Dr. Boxwell writes, that there "they have had but a few scattered cases in the town for the last six years, and not one fatal." In Arklow, Dr. Wright mentions that scarlatina has been very prevalent in that town and neighbourhood for several years past, particularly 1840—41; but it did not frequently prove fatal." In Athy, as appears from the letter of Dr. Clayton, it has prevailed, and some of the cases have proved fatal. Dr. Macartney, of Enniscorthy, states that it was prevalent and fatal during 1837 and 1838, and that it was, at the time of writing, breaking out again.

The communication of Dr. Ridley, of Tullamore, I will read at length.

"Scarlatina," he writes, "appeared here in the latter part of November as an epidemic, and continued to be very prevalent until June following. During this period it prevailed most in the month of March. I saw a great number of cases in this town and the neighbourhood, which were mostly all of the benign or simple form. Some cases occurred in full plethoric subjects, of an inflammatory nature; but I did not meet with a case of the malignant or typhoid disease, such as I have seen in Dublin. This epidemic raged chiefly amongst children and young people—the oldest subject I am aware of having had it was a person of forty years. It commenced with rigors, lassitude, loss of appetite (in some cases with soreness of the throat as a first symptom), and the usual symptoms of approaching fever, which continued to increase until the third or fourth day, during which time, in some instances, the fever ran high, with raving and other symptoms of cerebral disturbance. The eruption generally appeared on the second day in the form of small distinct spots like flea-bites, which did not run together, and declined suddenly on the fourth or fifth day without desquamation. In some instances the character of the eruption was an efflorescence, which remained out until the sixth or seventh day, and was followed by desquamation. The fever was equally high in both these forms of eruption, but of greater duration in the latter. The throat was very slightly affected in the majority of cases, being nothing more than a slight erythematous blush on the velum and tonsils; however, in some robust, plethoric persons, there was much inflammation, demanding active treatment. The symptoms had usually so much subsided as to enable the patient to leave the bed on the sixth or seventh day. *The fatal cases which I witnessed here were caused by congestion of the brain, occurring on the third day. While the eruption was well out and everything appeared favourable, slight drowsiness set in, which was quickly followed by coma and stertor;* and in two cases death ensued in thirty hours from the commencement of the symptoms: they were all in young persons of full habit, who had no previous delirium or inflammatory affection of the brain. The sequelæ were anasarca (which was very general, and occurred after the mildest form of the disease), pneumonia, bronchitis, acute rheumatism, remittent fever, and enlargement of the submaxillary and parotid glands. In one instance pneumonia proved fatal in eighteen hours. It was the case of a boy nine years old, who had been three weeks recovered from scarlatina. The treatment was that usually practised. Emetics and purgatives, diaphoretics, attending to ventilation, &c., were sufficient in the generality of cases. In the inflammatory form, venesection, antimonials, and calomel were prescribed; when the throat was affected, the free application of nitrate of silver was found to be the best remedy. Leeches, acid gargles, application of powdered alum, blisters, &c., were also beneficial. When anasarca followed it generally yielded to smart hydragogue purgatives; but in some cases I gave calomel and squill with advantage: as a prophylactic I was induced to try belladonna, but without success.

"There was at this time a very prevalent inflammatory affection of the throat, which appeared and disappeared with the scarlatina. This disease commenced with slight fever, stiffness of the neck and dysphagia, which afterwards increased to a great degree. The pharynx, tonsils, and velum assumed a deep scarlet hue, and were in some cases covered with patches of lymph, which could be raised off with a probe, like the membrane of diphtherite. The tonsils became greatly enlarged; also the parotid and submaxillary glands; the jaw became fixed, so that the teeth could not be separated; there were inability of swallowing, hurried breathing, and high fever. These symptoms increased to the fourth or fifth day, when the fever subsided with diaphoresis, the jaw became relaxed, copious salivation came on, and the ability of swallowing was in some degree restored; and, finally, in the course of eight or nine days from the commencement of the attack this inflammation ended in resolution. In some few cases one or both tonsils suppurated, and in other still rarer instances ulceration of the pharynx followed. Such are the symptoms of the most severe form of this disease; but it was sometimes so mild as not even to confine patients to the house.

"At any other time this disease would have been looked on merely as an epidemic cynanche; but in this instance there was a very decided connexion observed between it and the prevailing scarlatina. It was, in the first place, even popularly remarked, that a person who had suffered from this cynanche had not been afterwards attacked with scarlatina, and that an attack of the latter was not in any instance followed by one of the former. It was likewise observed that when one member of a family was seized with cynanche, scarlatina soon showed itself amongst some of the rest; and in the same manner, when scarlatina appeared first, cynanche very frequently followed, so that one was considered the forerunner of the other. The following few brief cases may serve to show this connexion.

"Case 1.—Master S. came home from school (where scarlatina had prevailed), complaining of soreness in swallowing, slight headache, and nausea. The next day the tonsils were enlarged, and he complained of greater pain in swallowing; pulse quick, skin hot; *but no appearance of eruption*. These symptoms remained, not getting worse, for three days, when they gradually subsided. Before he was perfectly well, scarlatina seized two of his sisters and his father. In the former, the eruption appeared as an efflorescence and ended in desquamation; in the latter it was in the form of distinct spots, and without any subsequent desquamation.

"Case 2.—Master O. came home from the same school with scarlatina. Two of his sisters and his brother were seized with it while he was ill. The eruption came out well in the spotted form. At the same time the man and maid-servant were attacked violently with cynanche, which was attended with high fever for several days.

"Case 3.—Visited Mr. B., who had been suffering from severe cynanche for four days. He cannot articulate or swallow; the jaw is so fixed as to prevent the teeth being separated to more than a quarter of an inch; fresh tumefaction of the neck; pulse quick; skin hot and dry; breathing hurried; face swollen and flushed; eyes suffused (on inquiring if any of the family had scarlatina, I found his son, who was lying in the same room, just recovering from it). After a few days perspiration appeared over the surface of the body, the fever became less, and he was able to open his mouth and swallow a little. On first seeing the tonsils and velum, I found them coated over with a thick white membrane, which extended to the hard palate, and could be raised off easily with a probe.

"Case 4.—P. N. has been complaining of headache and nausea since yesterday, feels a stiffness in his throat, and fears he is getting the scarlatina, as three of his children are only recovering from it. The throat symptoms increased to a great degree, with a smart fever attending them. No eruption appeared, and he was well in eight days.

"It is now almost generally admitted that the eruption is not a necessary symptom of scarlatina, which disease may occur independently of any affection of the skin. In this case the throat is supposed to be invariably affected, and the disease has received the name of 'scarlatina faucium.' But it may be a matter of some difficulty to diagnose this scarlatinous affection of the throat from a common cynanche: the fact of scarlatina being prevalent in the neighbourhood, and the probability of the infection of it having been in some way communicated, must in such cases be taken into consideration. If it be found, however, that exposure to the infection of one disease gives rise to the other, and that one proves a preventive of the other, there are fair reasons for concluding that it is the same disease, in the one case affecting the skin, and in the other the throat only."

In Waterford, Dr. Elliott announces that for several years scarlatina has appeared occasionally in an epidemic form, sometimes assuming great malignity during the congestive stage, whilst its peculiar diagnostic characters were as yet barely discernible. Dr. Bewley, of Moate, mentions that it has not prevailed in his district for eleven years, and that during the whole of this period he had not a fatal case. Dr. Thorpe, of Listowel, has seen very few cases of the disease, and has not had a single death. Dr. Gogerty, of Nobber (County Meath), has had many fatal cases, and the disease has been very prevalent. In Pomeroy, as appears from the statement of Dr. Harvey, the disease has been rare and mild.

Dr. Connor, of Carlow, writes as follows:—"I delayed answering your circular until I could send you the combined opinions of some other practitioners, two of whom agree with me in saying that there is annually a pretty general attack of scarlatina in this district, but nearly confined to the juvenile and infantile portion of the community; at least we do not recollect many adults affected with it, and only one fatal case amongst those, and that was the case of a lady just confined, and whose children had the disease, but recovered. As to the malignity of the type, we can say that, whilst five children were carried off by it in one family, others in the same house had it slightly; and although several lost two or more children, numbers of families have been so slightly affected that, were it not that medical men recognised the disease, it would have passed away without any notice, requiring in some cases only the little patient to remain one day or two in bed. When many members of one family have been taken away, we have had reason to think that the constitution of the sufferers had more to do with the fatal result than the original type of the disease."

Dr. Long, of Arthurstown, states that during the years 1841—2 he has not observed a single case of scarlatina in his extensive district, although at New Ross, which is but ten miles distant, the disease has prevailed in its most malignant form, and been attended with frightful mortality; but that, in the summer of the year 1839, scarlatina raged with him epidemically for some months. Its general character was at that time of a mild type, yet in some cases the malignant symptoms were present. He had then occasion to remark, that in the same family were to be found individuals presenting well-marked cases of every form of the disease, from the simple fever with bright efflorescence of the skin, to the sloughing tonsils and typhoid type; and that in many the disease appeared to attack the throat alone, presenting symptoms that would, under other circumstances, have been considered indicative of simple cynanche tonsillaris.

Dr. Russell, surgeon of the County Tipperary Infirmary, reports that in the year 1846 scarlatina was very prevalent during the spring, and assumed a most fatal form. It appeared also to be most infectious, as almost every individual, except those who had the disease previously, who came near the infected were seized with it. The fever was of a typhoid character, and the throat appeared as if affected with gangrenous erysipelas. The treatment that appeared most useful was warm bathing and the carbonate of ammonia with bark. He has had occasional cases of it since, but not at all of the same fatal form.

In Ballina, Dr. Whittaker says the disease has been rare and mild. Dr. Stewart, of Lifford, states that two epidemics have visited that district within the last six years: both were very mild. Dr. Croly, of Mountmellick, says "that scarlatina has at intervals prevailed in this locality for the last few years. Latterly it has assumed a malignant and fatal type, especially among children. The eruption was of a dark hue, with early tendency to spacelated ulceration of the fauces and pharynx, cerebral congestion with coma and convulsions."

From Dr. O'Brien, of Ennis, the following particulars were received:—"In reply to your circular relative to the prevalence of scarlatina in this county, I have not many observations to make, as it is only within the last seven or eight years that much of that disease has been seen here.

"My father, who has been forty years in practice in this county, told me that he has seldom seen the disease, and that it never prevailed as an epidemic here. About seven years since a few cases appeared here together, and it did not re-appear until the spring of the year 1840, when it broke out in a large school in this town, and four persons died of it. I was in attendance on them, and was seized with it myself, and had a very narrow escape. It was evidently brought to the school on this occasion by a boy who had just come from the King's County, and who showed the disease in a day or two after his arrival. It spread with such rapidity through the school, that (notwithstanding the greatest precaution) the establishment had to be broken up for some time. It again reappeared about Christmas in the same year, to a slight extent; was not fatal, and has not been seen since."

In Boyle, Dr. Hall says, it has lately been prevalent but very mild. Dr. Taylor has seen very few cases at Ferns, all of which were mild. Dr. Griffin, of Limerick, writes as follows:—"We had some bad cases of scarlatina in Limerick last winter (1841—2), and about two years ago, but they were few in comparison to the mild cases; and at any time within the last eight or ten years I have not known it to spread extensively as an epidemic. Those who died of the complaint suffered chiefly from the sloughing of the throat; but I saw one young girl die last year on the third or fourth day, apparently from the intensity of the fever and great prostration of strength."

From Dr. Roe, of the Cavan Infirmary, I received the following full and very satisfactory particulars. His letter was dated 29th August, 1842. "Scarlatina," he writes, "has been more than usually prevalent during the last few months. I have only seen two cases of it in adults. Amongst children I cannot say that those cases I met with were unusually severe or unmanageable. The soreness and swelling of the throat, with ulceration, were also very prominent and painful symptoms in several cases I saw; the sore throat appeared almost the only symptom, and the cutaneous affection very trifling and evanescent. I cannot say that the type of the disease here, as far as I met with it, was of a malignant character, nor did it put on that congestive inflammatory form which produces such an awfully fatal disease. I have also seen two or three examples of the dropsical or anasarcous symptoms which sometimes succeed; and in one very fine healthy child, which I had an opportunity of seeing the evening before its death, and of making a post-mortem examination, I found the entire cellular substance of the body pervaded with the dropsical effusion, and a very large quantity, amounting, I think, to nearly a quart, effused into the thorax. From the great dyspnœa, and very unequal action of the heart, I presumed there must be structural disease of the heart—which was not the case—and all the viscera, both of the thorax and abdomen, appeared perfectly sound, so that the dropsy was entirely the result or the consequence of the original disease three weeks before, and from which the child appeared to have perfectly recovered.

"Scarlatina, mixed up with small-pox, has appeared also in our poor-house, but not of a very fatal or malignant character; and I find from the physician of the poor-house that the scarlatina was rather of a low type, and required cordials, as wine, &c., and that bleeding was had recourse to only in a few cases. Some years ago, when it appeared in an epidemic and very severe form, I had an opportunity of seeing much more of it, and then I found the most beneficial effects from full and early bleeding;

and in two remarkable instances, one an adult lady and the other a fine healthy girl, I think it put an end to the disease, and prevented the congestive stage from coming on."

It is unnecessary to give the particulars of the many letters I have received on this subject; but from all may be collected the facts that scarlatina has, generally speaking, prevailed to an unusual extent in Ireland from 1836 to 1844—that it has, in many instances, been singularly rare in districts immediately adjoining others in which it has been equally prevalent—that there is no geological or physical difference in many of the localities alluded to, which can in any way account for these anomalies—that we are equally at a loss to explain its mildness in some districts in which it has extensively prevailed, and its malignity in others.

Even in this city, during the period of its greatest virulence, whole families have been attacked with the mildest forms of the disease I have ever seen; and I have been assured by many of the physicians connected with our dispensaries, that they have for a certain period met with several cases, all extremely mild; and suddenly the character of the disease has changed, and the cases then coming before them were as remarkable for their malignancy, and undue proportion of mortality. This was seen in a remarkable degree in the practice of Dr. Osbrey, physician to St. Mary's Dispensary, whose very important communication I shall read for you.

"The number of cases of scarlet fever," says he, "which were under my care, from the close of the year 1840, the period at which that epidemic first appeared in my dispensary district, until its decline at the commencement of the present year, amounted to somewhat above two hundred.

"When it first appeared the epidemic was of so mild a character that I treated above forty cases without the occurrence of a single fatal one; merely attending to the state of the bowels and secretions was sufficient to effect a cure. It soon, however, assumed a more formidable character. The cases which mostly proved fatal were those affected with diffuse inflammation of the neck; they were generally children under four years of age. As it may be interesting, I shall describe to the best of my recollection the progress of that affection, together with the treatment which I found to be most successful.

"Those cases of scarlatina in which this form of inflammation presented itself I was usually not requested to attend until some time after its commencement, which generally took place at the decline of the eruption on the third or fourth day. The attending, or I may say secondary fever, was principally marked by the occurrence of cerebral and nervous symptoms; the child either lay in a comatose state, or was excessively irritable and restless, and constantly whining. In those who were a few years older, a peculiar wildness of manner was occasionally observed, and if this were absent, the expression of the countenance was stupid and vacant. A common remark of their mothers was 'that they did not consider them in their right mind.' The children were affected with tremors of the extremities; the pulse was generally quick, and the tongue furred, but neither invariably so.

"The progress of the inflammation was very insidious, in most cases commencing as an indurated swelling behind the angle of the jaw on one side, which was at first very indolent, without any discolouration of the integuments; but as the affection advanced, the swelling increased much more rapidly, often extending to the opposite side: the integuments then assumed a dusky red appearance, and became very tender to the touch; there was much œdema, so that the part readily pitted when pressed by the finger, and there was an obscure sense of fluctuation communicated to the touch. In the advanced stage of the complaint, sensibility, which was previously great, diminished to such a degree that the child did not seem to suffer much pain if incisions were made into the swelling. When the patient survived till about the tenth day from the commencement of the affection, sloughs frequently formed, commencing in dark purple specks over the surface of the swelling; the sloughing rapidly spread; diarrhœa then set in; the abdomen became tympanitic; spots of purpura at times appearing over the surface of the body, with the occurrence of passive hemorrhage from the mouth and bowels. The child either died comatose or exhausted by diarrhœa, in case dissolution was not quickened by the supervention of sudden hemorrhage from some of the large vessels of the neck giving way in sloughing. An attack of convulsions sometimes preceded death, the period of which, unless when precipitated by the exhausting treatment to which it had been submitted previous to my seeing the child, varied from the seventh to the twenty-eighth day, the medium time being about the twelfth day from the commencement of the inflammation.

"In those cases which fell under my observation I did not notice anything differing from the common, either in the appearance or duration of the eruption, except that it was occasionally somewhat more faint than usual, and that the desquamation of the cuticle did not take place after its disappearance. Though what I have just described was the usual progress of inflammation when it ran a fatal course, yet sometimes it commenced earlier, on the first or second day of the eruption, the fever being of a more inflammatory type, the skin being hot, pulse strong, tongue furred, and much thirst being present.

"It is well known that this disposition to slough in scarlatina is not confined to diffuse inflammation of the neck, and that an inflammation of a similar character may attack other parts of the body. This was well exemplified in two cases which were under my care—one that of a boy, aged six years, in whom scarlet fever immediately succeeded an attack of hooping cough, for the relief of which I had found it necessary to apply a blister to the chest—the other that of a fine child, his sister, aged four, who had received a slight scald in the ham of the left leg; such portions of the vesicated surfaces which had not as yet healed were attacked by the inflammation, they were rapidly covered with ash-coloured sloughs, and the sloughing extended by livid margins. The boy, exhausted by the previous attack of hooping cough, which had been unusually severe, died; the little girl recovered. The treatment, which was the same in both, consisted, locally, in the ulcers being kept constantly covered with fermenting poultices, and in the margins being occasionally touched with strong muriatic acid. The cautious exhibition of mild tonics, and of stimulants, both diffusible and permanent, carbonate of ammonia, of course, being used amongst the former, constituted the general treatment. Another case, which I considered very remarkable, fell under my observation, *in which, simultaneously with the gangrene of the neck, sloughs formed on both corneas, which rapidly extended, involving all the other textures of the eyes.* This case my friend Dr. Battersby saw with me. The destruction of the eyes took place two days before dissolution. In each three of these cases the appearance of the part, when destroyed, closely resembled that of hospital gangrene. I did not notice any depots of purulent matter in the joints, or any other parts of the body.

"Having now described the more formidable characters of the inflammation, I shall state the treatment which I found most efficacious in arresting its progress, and in bringing about the more favourable terminations of resolution and abscess, and shall also take a short review of other modes of treatment that are occasionally resorted to. Mine simply consisted in the constant application of common oatmeal or linseed poultices, supporting the child's strength by nourishing diet, and in the cautious exhibition of permanent and diffusible stimulants; carbonate of ammonia, as I have already said, being included in the latter. The practitioner should not I think swerve from this line of practice, though he may sometimes be urged by the parents, alarmed at the progress of the inflammation, to treat the child more actively, particularly with respect to the local applications.

"When an abscess forms, the swelling previously diffused becomes more prominent, soft, and fluctuating, there is no pitting on pressure with the finger, and the surface is usually of a rose-red colour. Incisions may be then made to give exit to the matters. The symptoms of general disturbance, such as coma, convulsions, and such others as have been described in such cases disappear, tremors of the extremities alone remaining, and the child gradually recovers from the extreme debility from which it had hitherto suffered. I have said that stimulants should be given with much caution, for when used at all freely they are almost sure to induce convulsions, to which there is a great tendency throughout the progress of the complaint. After the formation of abscess, however, they may be exhibited with greater boldness. In cases where there is much restlessness and irritability, or when diarrhœa has set in, which usually does not take place until the sloughing has commenced, I have given opiates, either in the form of Dover's powder, or the pulvis cretæ compositus cum opio, the doses being carefully graduated according to the age of the child. When the cases became complicated with purpura and passive hemorrhage, it is almost unnecessary for me to say that I gave the mineral acids.

"I have ordered chloride of soda both internally and as a lotion. Its internal exhibition did not appear to be productive of any benefit, but as a lotion and gargle it was highly useful in destroying fœtor. When I apprehended internal sloughing, I occasionally touched the throat with muriatic acid lotions by means of a camel's-hair pencil or a piece of a sponge.

"Having had frequent opportunities of seeing the effects produced by the use of mercury, and also by local bleeding, in the practice of others, I carefully avoided having recourse to such methods of treatment myself; the former, with rare exceptions, inducing purpura, passive hemorrhages, and sloughing, the accession of which is so much to be dreaded, and which are so liable to supervene of themselves; the latter precipitating dissolution by increasing the disposition to coma and collapse.

"I have in some cases made incisions into the swellings extending beneath the fascia of the neck, but I do not consider it good practice in the case of young children, however useful it may be in that of adults, for the following reasons: when made in the early stage of the inflammation, they have a tendency to prevent the occurrence of either of those most favourable results—resolution or abscess; and when made in the advanced stages, unless when abscess or diffuse suppuration takes place, they are of no use; they do not appear to check the sloughing of the integuments. The parents, moreover, to whose feelings such practice is generally repugnant, are very apt to attribute whatever ill afterwards befalls their child to these incisions having been made; a consideration which I think should weigh with us in determining against the practice, when no good is likely to arise from it. In case, however, abscess or diffuse suppuration of the cellular membrane occur, we should not hesitate to make free openings. Diffuse suppuration of the cellular membrane, a result which I have only now alluded to, is almost as formidable, when the children are very young, as gangrene, inasmuch as they are scarcely ever able to bear up against the extensive suppuration, and consequently die of hectic.

"Blisters and mustard cataplasms, merely applied as rubefacients, did not appear to me to be injurious, but I did not place much confidence in them.

"It is obvious from what has been already observed, that we should be most cautious in making a prognosis when we meet this form of inflammation, as it often, when most mild at its commencement, subsequently runs a most fatal course, and when apparently of a most formidable character, terminates kindly. I have known many a practitioner, from mistaking its nature, supposing it to be nothing more than common scrofulous inflammation, to augur favourably as to its termination, when its subsequent course showed how very erroneous such an opinion was. The constitutional symptoms, however, even though we have no very clear evidence of the child having had scarlet fever, will, when carefully attended to, always enable us to form a correct diagnosis of the disease. Perhaps it is from not recognizing this disease, which is one of the most formidable consequences of scarlet fever, that such discrepancies have occurred in the returns made by practitioners of the relative mortality of scarlatina in their practice. The children are frequently not brought to them till all traces of the eruption have disappeared, and in some cases, where it has been so faint as to have escaped the observation of the parents, or even that of the physician. In such cases it is easy to conceive that it might be mistaken for a disease *sui generis*, and that the previous existence of scarlatina might be entirely overlooked.

"When sloughing of the integument has commenced, the chances of the child's recovery are greatly diminished, and when it is under a year old, it is, I think, almost hopeless. To have an opportunity of observing the sloughing stage, it is requisite that the child should survive some time. When death is precipitated by injudicious treatment, or if the child does not undergo any, dissolution occurs so early from the secondary fever, that no further local appearances than the more diffused swelling will be observed. I should here mention that, in the worst cases, there is no attempt at suppuration, and the part, when cut into, resembles somewhat the cut surface of a rotten apple.

"Even when resolution or abscess occurs, we must not be too hasty in giving a favourable prognosis; for there is occasionally much subsequent debility, which may lead to a fatal result.

"I have observed instances of this form of inflammation which succeeded other exanthemata. The following case is, I think, an interesting example. It is that of a child aged two years, that had an attack of the natural small-pox ten days previous to my seeing it, and had been treated by another physician, who, on the occurrence of the inflammation of the neck (I understood from its mother), despaired of its recovery; and indeed, when I myself visited it, I had no better hopes. It was then lying in a state of coma, with its head quite motionless and thrown back, a large diffused swelling being behind the angle of each jaw and extending down the neck. The pulse was remarkably quick and weak, the tongue furred, abdomen tympanitic. On the child being submitted to the mode of treatment which I have described, it recovered from the state of coma it was in, and two very large abscesses formed in the situation of the swellings, which I subsequently opened. The tympanitic state of the abdomen was relieved by injections of castor-oil and turpentine. When the sensibility and intelligence of the child returned, it was attacked with convulsions, consisting in constant motion of the extremities and twitchings of the face, which continued for two days. These convulsions I ascribed to debility, and treated as such. In this case purulent depots formed over the back of the hand and one foot; also a large one over the scapula. The two former have been absorbed; but as the one over the scapula still continues, and as there is no chance of its absorption, the recovery of the child is still doubtful. A few spots of purpura appeared in different parts of its body, and it has been frequently attacked with diarrhœa. Its mother states that it was a strong child before the attack of small-pox. It is, however, apparently of scrofulous habit, and had once suffered from rickets."

LECTURE XXIV.

SCARLATINA.—ABSENCE OF ERUPTION.—DROPSY AFTER.

ANY one who has studied our old authors with care, will perceive the identity that exists between *some* forms of our malignant scarlatina and the epidemics described by the name of the "*Ulcerous Sore Throat*,"* and the "*Putrid Sore Throat*."† But in these epidemics, the principal and fatal symptoms were inflammation and sloughing of the throat, ulceration of the schneiderian membrane, attended with profuse ichorous discharges, &c. The eruption was either disregarded, or only mentioned as a curious phenomenon, and death was produced by the "*sore throat*." But, as was before observed, many of our cases died without a single lesion that we could detect—*they were poisoned by the virus of the scarlatina.*

I shall now bring before you the particulars of a few fatal cases that were lately under my care, from which you will be enabled to form a better idea of the malady. The following notes were taken by Dr. Henry Kennedy, by whom the patient was first seen:—

Case 1.—"I first saw J. K., aged 14, on Friday evening, March 22, 1842. He had been at school the previous day, but had come home complaining of not being well. His mother had given him an emetic of ipecacuanha, which not only vomited but appeared also to have purged him. When visited about twenty-seven hours from the beginning of his illness, the purging had ceased, but the vomiting continued incessantly. He was throwing up quantities of dark greenish bile, and this occurred whether he took any drink or not. The thirst was insatiable, and the desire was for what was cold; the fever ran very high, the skin hot, pulse 140, and when left to himself he was inclined to rave; he referred his distress to the stomach, and said he believed his throat was sore; on looking into the mouth the internal fauces appeared inflamed, and the tongue was densely loaded, but there was nothing of that specific nature which would lead one to pronounce on the nature of the disease. At this time I looked very particularly to the state of the skin, but no eruption was visible. His position in bed was changed every moment, as he said for the purpose of giving himself relief. I had intended putting a few leeches over the stomach, but at the patient's own urgent request was induced to bleed him from the arm to about seven ounces. A sinapism was applied to the epigastrium, a mouthful of cold water given frequently, and the extremities sponged with vinegar and water. On the following morning (Saturday) all the symptoms had increased in severity, the treatment of the night before had only temporary effect, in fact for about four hours, after which all the symptoms had reappeared. In addition, the body was now covered with an eruption which could not be distinguished from the maculæ of bad typhus fever; it was best marked, however, on the chest and back; it was quite distinct on the face. The raving was now of a more decided character, and it was more difficult to make him give a direct answer. At this period Dr. Graves visited the patient, and recommended internal stimulants, with blisters to the surface. It is enough to add, that all treatment appeared to be quite useless, and from this time to the period of the patient's death, every symptom went on increasing, the raving becoming every hour more violent in its character, and the pulse rising to 170 and even 180. One or two points are, however, worthy of notice. During the last day of life the bowels were once affected, the discharge quite natural, and from this moment all vomiting ceased. During this day, also, a second crop of eruption made its appearance; it was perfectly distinct from the first, being of a reddish colour, and the spots much more circumscribed. I have often had occasion to meet with the same since; it was now that the tongue put on its characteristic appearance. That the nervous system was profoundly engaged there was but too much evidence of, for, though no convulsions came on, I observed strabismus, and the mouth was distinctly drawn to one side. There were also very violent fits of shuddering, almost amounting to rigor; the eyes were not at all injected. The entire duration of this patient's illness was about sixty-eight hours."

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* Huxham on Fevers, p. 266. London, 1772.

† Fothergill's Works, vol. i. p. 341. London, 1783.

In the patient whose case has been detailed, we have a remarkable example of scarlatina terminating rapidly in death, without the sloughing of the throat, which usually caused death in the epidemics narrated by Huxham and Fothergill.

Case 2.—Miss H., a strong healthy lady, aged 28, was attended at the commencement of her illness by Mr. Nicholls; when I saw her, there were intense redness of the throat, great dysphagia, and pain in swallowing. These symptoms induced me to bleed once freely; the blood was buffed and cupped to an extreme degree. After the disease had lasted for about thirty-six hours, an eruption of a vivid bright colour appeared. She obtained no relief from the bleeding, the pulse became quicker, debility increased, and she died with symptoms of poisoning in less than two days.

The occurrence of arthritis as a complication of scarlatina we have frequently witnessed in the Meath Hospital. In a man named Pierce, we had the greatest difficulty to save both wrist joints from ulceration. And in another case, the motion of the elbow joint was almost lost from the effects of inflammation.

I have noticed that when any of the viscera became engaged during the progress of this disease, that there is the greatest difficulty in subduing the local affection, and that it runs its course with great rapidity; this was unfortunately too well illustrated by the case of P. B., attended by Surgeon Smyly, of Merrion Square, and myself. The notes of the case were taken by Mr. Smyly:—

Case 3.—Miss P. B., aged 20, of a full habit of body, in December, 1841, was attacked with a very severe form of scarlatina. The eruption appeared on the 20th, and was very intense in its character; in the progress of the complaint her head became much engaged, requiring the application of leeches; her throat also was very bad, to relieve which leeches were again applied. Considerable prostration of strength accompanied the affection from the commencement. On the 30th she was so far recovered that I took my leave.

It may be worthy of remark that her sister, who was first affected with the disease, had it so mildly that the nature of the affection was not discovered, nor did she require medical aid.

January 9, 1842, I was again called to see Miss B.; she then complained of severe pain in her left side, which she first felt on going to bed, which became so violent during the night as to banish sleep. She had been till then going on as well as possible, recovering her strength daily, and was in excellent spirits. On the 8th, she ate heartily of beef-steaks for dinner, and drank some wine. I saw her sixteen hours after the commencement of the pain, when I found all the lower part of the left lung densely hepatized.

The treatment consisted in repeated cupping, attended each time with much relief, the exhibition of mercury so as to affect the mouth, antimonials; by all which means the acute symptoms disappeared, but no improvement took place in the condition of the lung. Her strength began to fail, and she died on the morning of the 19th of January, 1842.

In this case the most remarkable feature was the rapidity with which the lung was solidified, and the obstinate manner in which it refused to yield to treatment.

The following case was also attended by Mr. Smyly and I; it presents an usual sequela of scarlatina, namely, aphthous ulceration of the anus, which, though not sufficiently pointed out in the late works on practice of medicine, was recognized and described by Huxham, and I introduce our case with Huxham's description, to show more clearly the identity of the two epidemics.

After alluding to the profuse discharge that took place from ulcerated surfaces in the mouth and nostrils, he says:—"A sudden stoppage of this rheum from the mouth and nostrils actually choaked several children; and some swallowed such quantities of it, as occasioned excoriations of the intestines, violent gripings, dysentery, &c., *nay even excoriations of the anus and buttocks*."*

Case 4.—Master James F., aged 12, was affected with a very severe form of scarlatina, in July, 1841. The eruption began to appear the second day of his illness, and became very intense in its character, the whole surface of the skin being almost of a uniform redness. The accompanying fever ran very high, demanding venesection to reduce it. The throat presented the usual appearance; but in this case the inflammation extended into the mouth, and we suspected throughout the intestinal canal, indicated by the great irritability of the stomach and bowels, and the *circumstance of the anus presenting the same aphthous appearance that the mouth did.* The inflammation also extended into the left ear, and caused the destruction of the membrana tympani. In less than a month's time this young gentleman was so far recovered as to return to England, and has since enjoyed good health.

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* Huxham on Fevers, p. 280. London, 1772.

In the writings of Huxham and Fothergill you will also find frequent allusions to cases where death was produced by uncontrollable epistaxis, during the epidemic of "sore throat" described by these authors. Thus Fothergill, after speaking of the usual mode of death in these cases, says:—"Though this was the common progress of the disease where it terminated unhappily, yet it often varied from this type, and was attended with very different symptoms. Some had an extreme difficulty of breathing almost from the first; some had a violent cough; some were comatous; others had a delirium; some died in a lethargic stupor; *others bled to death at the nose.*"* The following is an example of this form of the disease:—

Case 5.—I was called to see the Rev. Mr. C., aged 25, of regular temperate habits, and healthy constitution. He was then labouring under severe fever with sore throat. On examining the fauces I found the tonsils extensively ulcerated. These were touched with nitrate of silver, and the next day they appeared much improved. On the third day of his illness an eruption appeared, neither too red nor too pale; in short, as favourable as could be wished for, and perfectly normal as to its duration. During all this time the heat was intense; and on the third day of the eruption the cold affusion was employed, and was followed by marked relief; but the pulse still remained sharp and quick, never falling below 96. In this state he continued till the seventh day of his illness, when epistaxis occurred (to this he had been subject for a long time), and was followed by considerable relief of his head. The epistaxis was not excessive, and from the fact of its being habitual excited but little alarm. About the thirteenth day the fever had almost gone; his sleep was good, and his tongue moist and clean. At this time a small tumor, situated at the angle of the left jaw, and which had been there from the beginning, was observed to enlarge. The next day it had spread considerably, was very red and painful. The fever increased, the tongue became dry, and his sleep was disturbed. On the sixteenth the tumor was examined and opened by Mr. Cusack.

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* Fothergill's Works, vol. i. p. 353. London, 1783.

A large quantity of good healthy pus escaped, and the patient experienced great ease. On the eighteenth day a deeper incision was made by Mr. Cusack, and again a large quantity of good pus escaped; but on this occasion no relief followed. On the next day the constitutional symptoms were much more severe; the epistaxis returned, and the tongue was now dry, black, and bleeding. There was no raving, nor was he at any time during the illness in the least delirious.

Notwithstanding that the nares were plugged, and every measure which Mr. Cusack and I could think of employed, the epistaxis continued, the bleeding from the tongue could not be arrested, the tumor in the neck became gangrenous, and on the twentieth day of his illness death terminated his sufferings.

The next very interesting case was communicated to me by Professor Porter, and as it illustrates another sequela of scarlatina, I shall read it for you. There can be no doubt that the hemorrhage originated in the way pointed out by Dr. Porter, and it shows in a convincing manner the assistance in diagnosis which we derive from an accurate knowledge of anatomy. It will also be recollected, that this case differs in the manner in which the bleeding occurred, from that mentioned in a preceding lecture by Dr. Geoghegan of Kildare. The older writers make frequent allusions to examples of this latter form of hemorrhage.

"On or about the 18th of September, 1841, Master ——, aged 11, was attacked with scarlatina. He was of remarkably fair complexion, thin, almost transparent skin, and hair nearly white. The disease assumed rather a mild form, the eruption came out abundantly, and began to disappear about the evening of the fifth day. The throat was slightly engaged—very little difficulty in swallowing; but there were three or four external tumors, exactly resembling scrofulous glands about to suppurate, and there was discharge of puriform matter from both ears, with slight deafness of the left.

"At the end of about ten days he seemed to be progressing favourably as to health; two of the little glands on the left side suppurated and were opened, discharging healthy pus; his appetite was pretty good and his sleep sufficient, but he always rested during the day, and remained awake during the night. The discharge from the ears continued, and he had become quite deaf at the left side.

"Soon after (I cannot be particular as to dates) a gland on the right side suppurated, and was opened. The discharge was healthy; but that from the right ear began to be thin and abominably fœtid, the smell evidently indicating its connexion with some diseased bone. He was now totally deaf of both ears. Our communications with him were altogether by signs, and he was becoming paralytic on the right side of the face; all his features being drawn frightfully to the opposite side when he either laughed or cried. At the end of about six weeks, however, he seemed in some respects to be greatly improved. He slept well, he was exceedingly cheerful and even lively, and his appetite almost voracious. He had so far recovered his hearing, that we could communicate easily with him; but the foul discharge continued in great abundance from the right ear, and the paralysis of the face increased. He continued apparently to improve in general health and even in strength until the end of the ninth week, and had occasionally been up and dressed for a few hours in the day, when in the middle of the night a new symptom appeared.

"The child had been asleep, when he suddenly awoke, screamed out 'Oh, my ear, my ear!' when almost instantly a gush of blood took place from the right ear. This blood was florid, and had the appearance of being arterial. It came gushing forth most profusely, as if poured from the lip of a jug or ewer, and was sufficient in quantity to soil several towels before it ceased, which it did rather from the exhaustion of the patient, than from the means employed to control it. I was not called during this first bleeding, but saw him early next morning. He complained of dreadful pain in the left side of the head, resembling hemicrania. The discharge from the ear was a thin fœtid serum, mixed with flakes of unhealthy matter, and discoloured with blood, and the paralysis of his face greatly increased, the features being distorted even when at rest. I attempted to plug the ear; but the pressure interfering with the discharge of matter, caused intense pain, and could not be used even for a few minutes. From this time he continued to bleed at irregular intervals until his death; and as I was present on three or four occasions, I shall endeavour to describe one of the attacks of hemorrhage.

"He seemed to have no previous warning whatever: sometimes the bleeding commencing during sleep, sometimes while he was amusing himself with his toys. He generally gave a single scream at the instant, and then the blood burst forth with a gush that really astonished me. I never saw blood lost so rapidly in any surgical operation I ever witnessed, and only once in an accident where the deep jugular vein had been opened. This bleeding could hardly be controlled by pressure, and the attempt to do so caused intense pain, so that at times the nurse did not interfere, but allowed it to stop spontaneously, which it generally did in about a minute. The blood was always florid. The attacks occurred at irregular periods, and there never was hemorrhagic fever.

"About a week before his death, I observed that the blood was beginning to make its way by the eustachian tube into the pharynx, some of which passed into the stomach, and some was expelled by the mouth, and then he bled sometimes by one passage, sometimes by the other, and occasionally by both. I need not say that he became pale, ex-sanguine, and exhausted, except to express surprise that any child of his age could have endured so long. The palate and inside of his mouth were as pale as any part of the external surface of his body. Exactly at the end of the thirteenth week from the commencement of his illness, he died after a slight gush of blood.

"There was no post-mortem examination, and knowing the feelings of the child's parents I did not ask it; therefore the pathology of this case must be matter of conjecture. I think there can be no doubt there was caries of some portion of the base of the skull, and from the symptoms, I always imagined it to be seated in the petrous portion of the temporal bone. The spot at which the carotid artery enters this bone is immediately adjacent to the bony portion of the eustachian tube, and it is probable that this latter was the original seat of the disease from which it spread until it implicated the vessel. The extraordinary size of the stream satisfied me of its being furnished by some large vessel; its colour showed it to be arterial; its escape by the ear, and afterwards by the mouth and nose, proved

This book is the [illegible]
COOPER MEDICAL COLL[illegible]

its passage by the eustachian tube; and I know of no vessel that would be sufficient to explain all the symptoms, unless the one I have mentioned—the internal carotid."

With reference to the diffuse inflammation of the neck that follows scarlatina, as described by Dr. Osbrey, my experience accords with his; and the recommendation to direct all our efforts to the support of the patient until the period of sloughing arrives, deserves the greatest attention. The following case occurred a short time ago at the Meath Hospital:—A child four years old was admitted on the fourteenth day of its illness, with the integuments in the front of the neck in a state of gangrene. In a day or two the sloughs separated, leaving the muscles of the neck completely bare, and as distinct from each other as if dissected. The common carotids were also laid bare, and could be seen pulsating at the bottom of the ulcer. A few days after, granulations sprung up, and the ulcerated surface soon cicatrized. I have not been able to ascertain if any, or what amount of contraction of the neck followed the healing of the ulcer.

Sir H. Marsh and I attended, not long since, a lady who had been affected for some days with fever and sore throat. She had no eruption on any part of her body; but from the character of the fever, and the peculiar appearance of the throat, we suspected she was labouring under an attack of scarlatina. Her family were very anxious to ascertain the precise nature of her complaint; and I visited her twice a day for the first four or five days of her illness, carefully examining the skin at each visit, but could not discover the slightest trace of an efflorescence of any description. She continued for several days to suffer from the fever and sore throat, and was at one time in a dangerous condition, but ultimately recovered by great care, and the use of appropriate remedies.

Now I watched this case from the sixth hour after its commencement to its termination, and repeatedly examined the skin, particularly that of the breast, abdomen, and inside of the knee and elbow joints, places in which the eruption shows itself when it appears at all, but could not discover any vestige of it. You will often find a diffused redness about the knees and elbows in cases where the eruption does not appear on any other part of the body; but in this instance there was not the slightest deviation from the natural hue. Yet the result proved that it was scarlatina; for the desquamation of the cuticle, which always attends this disease, took place, and the lady communicated the infection to several members of the family. A young gentleman residing in the house got a bad attack of scarlatina; two of the servants were also attacked, and the lady's father got sore throat; in fact, there could be no doubt as to the nature of the disease. During her convalescence she had desquamation of the cuticle; and this is a point to which I would particularly call your attention. We are taught to look upon desquamation as the result of cutaneous affections of an inflammatory character; and it is an opinion very generally maintained, that in scarlatina, as in psoriasis, the peeling off of the cuticle depends on the peculiar state of the skin produced by inflammation. It is stated that the increased vascularity of the skin occasions a morbid secretion and subsequent detachment of the epidermis, and that the same phenomenon is observed in all cutaneous affections of an inflammatory character. This may be generally, but not universally true; for here we had an extensive desquamation of the cuticle without any eruption, without any previous redness, pain, or remarkable heat; in fact, without any of the phenomena which are regarded as constituting inflammation. This seems to prove that there is something more than inflammation concerned as preparatory to that process which is termed desquamation, and the change which the skin undergoes is not to be looked upon as a mere consequence of inflammation occupying the external surface of the corium.

I observed another curious fact in this lady's case. Since the attack which I have just described, she has been shedding her nails; that is to say, the nails of the fingers are all dropping off, and yet there is no appearance of inflammation of any kind about the hands to explain the occurrence. You are, of course, all aware that the dropping off of the nails is a species of desquamation. From the peculiar structure of the nail, and the mode in which it is formed in the matrix, it does not drop off at once like a scale of epidermis; still I think we are authorized in looking upon the shedding of the nails as a species of desquamation. This affords a very curious subject of investigation, as connected with the history of fever. It is an opinion entertained by many persons, that desquamation of the skin takes place at a particular period of typhus; and that this is not an occasional, [illegible] This [illegible] Dr. Perry of [illegible] period in which [illegible] desquamation of the [illegible] more contagious [illegible] disease. [illegible] How true it may be, my [illegible] decide; nor am [illegible] of the occurrence. [illegible] that the occurrence of [illegible] of scarlatina [illegible] as to the connection [illegible] I think, at least, that [illegible] very different from [illegible] desquamation [illegible] with the ordinary process [illegible]

[illegible] my lectures [illegible] of this form of scarlatina, [illegible] efflorescence. They [illegible] between the ages [illegible] cases exhibited a [illegible] quickness of pulse, [illegible] urinary secretion, and, [illegible] depression, which continued [illegible] to treatment. The [illegible] red, and injected. [illegible] tonsils, and upper part of the [illegible] of a very peculiar dark-red [illegible] equally diffused over the [illegible] as far as it could be [illegible]

[illegible] very lately communicated [illegible] eminence in this city, is [illegible] scarlatina broke out in this [illegible] all his children with the excep- [illegible] in constant attendance on [illegible] did not exhibit any symptoms [illegible] When all the children had become [illegible] to the country for the benefit of air, whither she also accompanied them. Here she was, much to the astonishment of her family, attacked with the peculiar anasarca observed in persons who have recently laboured under scarlatina. Her father, under whose observation she had been during the whole time, was very much struck with the occurrence; he paid particular attention to the case, and feels convinced that it was the result of latent scarlatina. This case, connected with those already detailed, is of great interest in a general pathological point of view. It appears to prove the fact, that in some instances diseases produced by contagion do not give rise to the whole train of phenomena by which they are ordinarily characterized.

Let us turn for a moment to some of those diseases caused by the action of animal poisons on the system, as, for instance, measles. The symptoms which generally attend and characterize measles are universally known. After an attack of fever, on the third or fourth day, coryza, sneezing, hoarseness, and cough are complained of, and then a rash appears, first on the face, and afterwards on the body and limbs. But it is not necessary that all these symptoms should appear, and that the sequence of morbid phenomena should be uninterrupted throughout. On the contrary, it frequently happens, at particular periods and in certain constitutions, that some of the most usual symptoms are scarcely observed or altogether absent. You will find this point insisted on by Dr. Bateman, who has given a detailed description of a form of measles in which the catarrhal symptoms are wanting, and which he has termed rubeola sine catarrho. Thus we may have pneumonia without cough, and pleuritis without pain in the side. Those who have witnessed the course of epidemic cholera in this country, will recollect that many cases occurred in which vomiting, purging, or cramps were not observed.

If we turn to fever, we find that the animal poison to which it owes its origin generally exhibits a certain number of symptoms congregated together, or observing a determined order and succession; and these we meet with in most of the cases which come before us in practice. But we now and then see fever patients in whom one or more of the most prominent symptoms are absent. Thus occasionally there is no quickness of pulse or appearance of vascular excitement; in some there are no cerebral symptoms; in others no increase in the temperature of the skin. Indeed, I might go through the whole group of symptoms which accompany fever, and show that almost every one of them may be occasionally absent, and yet the fever of a severe and dangerous type. I recollect pointing out to the class last year the case of a man labouring under chronic enlargement of the spleen. He had been working for two or three seasons in some of the marshy districts of England, and had been occasionally ill, but never had symptoms of regular intermittent; in fact, he had escaped the intermittent itself, but not what are usually deemed the consequences of it. We have been in the habit of explaining the enlargement of the spleen by referring it to the conflux of blood towards the internal organs, particularly the liver and spleen, during the cold stage of intermittent; and we have endeavoured to explain the subcutaneous œdema which follows scarlatina, by attributing it to previous inflammation of the skin and subcutaneous areolar tissue; but the observations and facts which I have now brought forward will show that these opinions were founded on erroneous ideas.

Turning to cases of chronic disease, we find in some, as for instance syphilis, that the poison taken into the system gives rise in most cases to a determinate order of symptoms, *e.g.*, bubo, sore throat, eruptions on the skin, nodes, and syphilitic cachexy. Mr. Hunter has been at great pains in determining the order of the parts, and pointing out the tissues which are successively affected, and it is of considerable importance to have correct notions on this point. But, although the number and order of symptoms marked out by Mr. Hunter and others may be observed in most cases, they are not so in all; and the same remark which has been made on the occasional absence of one or more important symptoms in scarlatina will apply with equal force to syphilis. Now, when this morbid poison which excites syphilis does not affect the constitution in such a manner as to occasion the production of all the symptoms which usually characterize this disease, a variety of venereal is formed which often proves a source of great embarrassment, not only to the young and inexperienced, but even to the senior members of the profession.

It is of great importance, in a practical point of view, to bear in mind the general proposition I have announced, viz., that in both acute and chronic diseases *a constitutional affection may* *display its existence by only one or two of the numerous symptoms which usually accompany it;* and this occurrence seems more frequent in the case of diseases produced by contagion and morbid animal or vegetable poisons, than in the case of maladies generated by causes developed in the system itself.

The case of William Young, who was admitted on Wednesday last, has some claims to your attention, and demands a few observations on my part. This boy, who is about twelve years of age, had an attack of scarlatina some time ago, and had been dropsical for a week or ten days at the period of his admission. He was somewhat feverish, had thirst, heat of skin, and slight headache, cough, and difficulty of breathing, and, on making an examination with the stethoscope, we detected numerous bronchial rales; his lower extremities were anasarcous, and he had some effusion into the peritoneal sac. We could not ascertain exactly the time when this train of symptoms commenced, but it is very probable that it was a week or ten days after the disappearance of scarlatina. When patients who have been recently labouring under an attack of scarlatina take cold, the anasarcous symptoms appear in a very short time after the attack; but, even where they are not exposed to cold, the dropsy appears generally about ten days or a fortnight after scarlatina, and is very often accompanied by some pectoral affection. The disease sets in with febrile exacerbations more or less marked; anasarca of the extremities is next noticed, and at the same time the patient has slight cough and difficulty of breathing, which generally proceeds from congestion of the bronchial mucous membrane, but may be the result, though less frequently, of pleuritis or pneumonia.

If called to a case of this kind in the commencement, and where the patient is not greatly exhausted by previous disease, the treatment is exceedingly simple. By opening a vein in the arm, and abstracting a quantity of blood proportioned to the age and strength of the patient, you remove the inflammatory state of the constitution, and arrest at once the anasarcous and pectoral symptoms. It may occasionally happen that active measures of this kind cannot be taken, in consequence of the great debility of the patient from previous disease; but, generally speaking, cases of anasarca after scarlatina bear antiphlogistic treatment well. It is not after cases of violent scarlatina, or where the patient's life has been in imminent danger, that the supervention of dropsy is most commonly observed. The majority of dropsical cases of this kind are met with in patients who have had the disease mildly, and without any remarkable intensity either of the local or general symptoms. Hence venesection is borne well, and its performance attended by the most decided good effects, particularly where the dropsy is complicated with pleuritis or pneumonia.

In the case before us, however, being uncertain as to the exact duration of the disease, and finding several symptoms present indicative of weakness, we were obliged to proceed with more caution. The boy had been ill a week, and appeared to be under the influence of digitalis administered before his admission, for his pulse was intermittent and wavering. Under these circumstances, I determined to limit the antiphlogistic measures to the application of a few leeches over the abdomen. I did this with less hesitation, as an accurate examination of the chest showed that there was neither pleuritis nor pneumonia present. The internal remedies were calculated to increase the secretion from the kidneys. The boy's urine was remarkably albuminous, and of the specific gravity of 1027. This is a point worthy of remark. In many cases of dropsy after scarlatina, the urine is albuminous. Now, almost every case of this kind will get well, and, as convalescence progresses, you will observe that the urine ceases to be albuminous. These facts, of the truth of which I can speak with the fullest confidence, are quite sufficient to show that those persons are wrong who assert that albuminous urine is always the result of organic disease of the kidneys. Albuminous urine is here, as Dr. Blackall observes, merely an indication of a peculiar inflammatory condition of the whole system, and not of degeneration of the kidneys.* I may observe, however, that this is not invariably the case; for I could point out examples where albuminous urine is connected with an apparently opposite condition of the system, in fact, a condition demanding the use of generous diet and tonics.

Hence there must be great diversity in the treatment of dropsy with albuminous urine. Where it occurs after scarlatina, and is accompanied by febrile symptoms, it is best treated by the lancet, nitre, purgatives, and digitalis; but where it occurs in chronic cases, without any remarkable excitement of the vascular system, without organic disease, and with more or less debility, it requires to be treated with tonics, generous diet, and full doses of opium. In the present case I only applied a few leeches to the belly, and kept the bowels gently open for the first few days, being determined to wait until the pulse became regular before I ventured on any decided plan of treatment. I then ordered mercurial frictions to the abdomen and axillæ, and gave mercury internally, combined with small quantities of digitalis. He also got a draught twice a day, composed of carbonate of soda, tincture of squill, and syrup of orange peel. These remedies we shall continue for some time, carefully watching their effects.

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* These opinions have been since advocated by Dr. Burrows, in his admirable essay on Scarlatina, published in the *Library of Medicine*, vol. i., and which I feel great pleasure in recommending.

From the state of weakness this boy was in at the period of his admission, and the length of time the disease has lasted, I have not thought it advisable to bleed him. When cases of this kind become chronic, they are very difficult of cure, and require very delicate management. You will frequently have to run through the whole list of remedies employed on such occasions, before you can hit on one that proves successful. I recollect a case of this kind, in which the anasarca was extreme, and the boy's legs were enormously swollen; the dropsy was accompanied by scanty secretion of urine, but without any distinct febrile excitement. After having used every remedy I could think of for nearly three months, without any benefit, I resolved to try the effects of cold affusion, from which I had experienced much advantage some time previously in another case. I ordered a large vessel filled with pump water, in which a quantity of salt had been dissolved, to be poured over him twice a day, for the space of two or three minutes each time, immediately after which the boy was wiped perfectly dry and put to bed. The good effects of this measure became soon evident; a copious discharge of urine took place, the swelling of the limbs subsided, and in about six or seven days the child was able to run about as usual.

This case went on unfavourably, and the boy died, after lingering several weeks in a state of extreme dropsical swelling, and great suffering, distention, and dyspnœa. As his urine continued highly albuminous throughout, we were excessively curious to learn what was the condition of his kidneys. The post-mortem examination was made a few hours after death, and the kidneys were found in every respect healthy; their size, shape, consistence, and colour were perfectly normal. The long-continued presence of albuminous urine, in a case where no such state of kidney existed, forms conclusive evidence that this state of urine is not necessarily the result of that renal degeneration first described by Dr. Bright; the occurrence of one positive exception is sufficient to disprove such a conclusion, even though supported by a thousand cases, and consequently, when albuminous urine in chronic dropsy is found to occur along with Bright's kidney, I consider this particular state of urine and of kidney as depending upon different causes, which often co-exist in chronic dropsy, and consequently I regard albuminous urine as a sign of Bright's kidney, but not as its result.

It has been already observed that anasarca seldom occurs after severe and dangerous scarlatina, but it is not unfrequent as a sequela of the very mildest forms of that disease; a fact of which every practitioner should be aware, and a knowledge of which should prevent us from pronouncing a patient out of danger until the period during which dropsy may supervene is passed. To impress the necessity of caution, I may mention that I have seen several cases of scarlatina in young persons and children so mild as not to require confinement to bed, and yet followed about the eighteenth or twentieth day by anasarca; this usually yields to treatment without much trouble, but in some patients, and without our being able to assign any cause for it, the anasarca increases rapidly, the pulse rises, and in a few days is excessively rapid, from 130 to 150, becoming hourly weaker and weaker, while the heart's action is strong and tumultuous; the skin is hot, and in many individuals inflammatory symptoms manifest themselves in the head, chest, or belly, and the patient is carried off by internal inflammatory effusion into one or other of these cavities. Other cases are more treacherous, and the approach of danger is not indicated by anything but the rising of the pulse, and the rapid increase of the dropsical effusion, soon to be followed by convulsions that succeed each other until death closes the scene, a termination so much the more unexpected, as these cerebral symptoms have not been preceded by the least headache, or any perceptible affection of the functions of the brain!

In addition to the remedies already mentioned, I can speak with the greatest confidence of the utility of hydriodate of potash in the form of anasarca we are now treating of; and I may add, that I have found the following line of treatment more successful than any other in the malignant forms of scarlatina—local bleeding by leeches when necessary, wine and carbonate of ammonia freely given, with camphor mixture. In some cases, attended with intense heat of the skin, the cold affusion has given great relief; in others it has failed.

LECTURE XXV.

INTERMITTENT FEVER.—DISEASES WHICH SIMULATE IT.—THE MALARIOUS FEVER OF AFRICA.

I PURPOSE devoting to-day's lecture to the consideration of some points connected with intermittent fever, most of which I was the first to observe and describe. I will first read for you the notes of a case of intermittent fever reported by Mr. Power:—

"Mary Gannon, aged 44, was attacked by intermittent fever about the middle of September last. The paroxysms occurred twice every day, one in the morning, the other in the afternoon, for the space of ten days, after which, owing to medical treatment, the evening one disappeared. On the 10th of October she was admitted into the Meath Hospital, and was placed under the care of Dr. Stokes, who prescribed small doses of sulphate of quina, under the use of which the fit became tertian, but soon afterwards returned to the quotidian form. On the 1st of November she became a patient to Dr. Graves, and was put on large doses of the sulphate of quina. On the 7th of the same month the fit again assumed the tertian form, in which state it continued until the 17th, although the dose of quina had been increased to a scruple and a half in the day. She was then blooded to ℥xviij., by which the duration of the paroxysm was lessened, and the interval between it and the succeeding one increased by twelve hours. She was again blooded, and the fit become quartan. Venesection was repeated for three times, but without any other sensible effect than a curtailment of the duration of the existing paroxysm. Her strength now became reduced, and she was ordered to take four drops of the liquor arsenicalis in half an ounce of mint water, three times a day. Since she commenced taking the arsenic, the violence of the paroxysms has been gradually subsiding, and strength and appetite are returning; at present the fit presents scarcely any other characters than those of a slight shivering."

Now, what is the definition of a quartan ague? According to Cullen, it consists of "paroxysmi *similes* intervallo septuaginta duarum circiter horarum; accessionibus *pomeridianis*," that is to say, the attacks must be similar, there must be an interval of seventy-two hours between them, and the fit is to come on in the afternoon. Let us examine how far the characters of the present case coincide with this definition. Latterly, she had seven attacks with a precise interval of seventy-two hours; in the next place the attacks were similar; so far, so good; but the accessions of her paroxysms were in the forenoon and not in the afternoon, for they generally came on about eight o'clock in the morning, and in this respect accommodated themselves to our convenience, for we could be here to witness them. It is very true that we generally find the paroxysm of quotidian in the morning, of tertian in the middle of the day, and of quartan in the evening, and also that one may pass into the other; but to this I do not attach much importance. Here the disease evidently terminated by becoming quartan. A question arises as to what was the nature of the fever in the commencement? Was it any variety of quartan? That is, was it quartan disguised under the type of any other species of intermittent? In the beginning she had two paroxysms every day, constituting what has been termed the quotidiana duplex, a disease which is common enough, though it has not been noticed by Cullen in his Nosology. The nearest approach which the first form of our case makes to the acknowledged quartan of authors, is to the quartana triplex, where we have the fit coming on three times a day, with every fourth paroxysm similar. But you perceive plainly that Gannon's fever, in its first form, is not reconcilable to any known type of quartan ague.

Now, what was the effect of the remedies employed? First, to make it assume the form of a simple quotidian, and, as a still further improvement, resolve this into a *tertian*. Here we have an argument against the supposition of a concealed quartan, for an interval of 48 cannot, by doubling, be converted into an interval of 72. But the effect of remedies, nevertheless, produced this anti-nosological conversion, for the first bleeding in the cold stage made an addition of 12 hours to the tertian interval; and a second bleeding added another 12 hours, and then we had the quotidian interval complete. This was

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[illegible] the case; secondly, when
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[illegible] in consequence of the
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[illegible] the aguish fits entirely and at
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[illegible] such as we have described
[illegible] complete cure be effected,
[illegible] it is not a true tertian at
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[illegible] of twelve hours, and
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according to the laborious investigations of Nick, have shown that a regular revolution, as to its frequency, takes place every twelve hours, and the same result has been made with regard to the intensity of the respiratory process. We all know that there is a considerable difference between the nervous and calorific powers of the body during the twelve hours we spend in active employment and awake, and those which are chiefly passed in tranquillity and repose.

As the average period of day and night respectively is twelve hours, in the same manner equivalent spaces of time seem to be destined for the successive and alternating revolutions of the living system. It would be extremely interesting to consider what influence their adoption might have in our calculations concerning the crisis of continued fevers. We would not then count three days and a half, but seven half days; we would not say seven days, but fourteen half days. If this method were adopted, many of the apparently anomalous critical effects and critical terminations, in continued fevers, would, I have no doubt, become strictly conformable to some regular law of periodicity. To arrive at a knowledge of this law would be of the greatest importance, and would tend much to render our knowledge of fevers more accurate, and our treatment more efficacious. Those who entirely deny the critical period must be either very superficial observers or very indifferent practitioners. In private practice, where the precise commencement of the attack can be ascertained, a *crisis*, or an obvious attempt at a crisis, takes place often on the reputed critical day, occasionally on others; and if the treatment be judicious, it seldom happens that a fever terminates without either. Within the last year I have seen two cases in which a decided and perfect crisis took place on the forty-second day. In another case the salutary crisis took place on the thirty-fifth day. The first of these cases I saw along with Dr. Stokes; the second, with Dr. Plant; the third, with Mr. Rumley. In another case, which I attended with Mr. Kirby, there was an obvious but unsuccessful effort at crisis on the seventh, fourteenth, twenty-first, twenty-eighth, and thirty-fifth days.

I must admit that I have seen perfect crises on days not reputed critical; but I am convinced, that if the method of counting by half days and not by days were adopted, the exceptions to the occurrence of crisis would be much less numerous. The nature of a crisis has, I think, been never truly explained; to me it appears evident that all the phenomena which attend this curious change prove that when a continued fever terminates by crisis, *it is by being converted into a fever of a new type and shorter duration.* A well-marked crisis comes on almost like a fit of the ague; it is ushered in by great collapse, coldness, and even sometimes by rigor. This is succeeded by a hot fit, and that again by a sweating stage, copious deposition in the urine, &c., and then the patient is found free from fever. Is it not probable, therefore, that the crisis is not merely the termination of the former fever, but a new fever, as it were, superadded to it, for the purpose of exciting a change in the system, attended by such a powerful action of another kind, that the former chain of morbid actions is broken, and the tendency of the new fever to terminate in health is thereby allowed to prevail?

To many, I am aware, what I have said may seem fanciful, but to a close and candid observer of nature this hypothesis may not appear altogether unfounded.

I shall not detain you, gentlemen, in making any remarks on the treatment pursued in Gannon's case. You have seen how the sulphate of quina changed the type of the fever, and you observed how completely the *liquor arsenicalis* succeeded in removing the disease, after other remedies had failed. It is to be recollected, however, that considerable advantage was derived from venesection in the cold stage, and it is probable that this treatment by the lancet was a useful preparation for that by arsenic. It has been supposed that bleeding, during the cold stage of ague, produces a favourable effect, in consequence of its relieving the internal sanguineous congestion. This hypothesis, however, does not appear well founded, for the utility of venesection is by no means confined to those cases of intermittent fever, in which the cold stages are attended with an evident diminution in the external circulation, denoted by a shrunk countenance, cold and pointed nose, and a pale corrugated skin. In such cases it is very reasonable to conclude, that the internal organs must labour under sanguineous congestion, as long as the quantity of blood in the periphery of the body is diminished; but this obvious deviation from the proper balance of the circulation is not observable in every case; and in that related above, the temperature of the external parts was increased at the very moment that the violence of the rigor was greatest, while at the same time the extremities, face, and general surface of the skin appeared to enjoy a more than usually abundant and active circulation. We must, therefore, refer the benefit derived from the venesection to some other cause, most probably its energetic action on the nervous system; it is to this we must attribute its effects in stopping the rigor and lengthening the intermissions.

That the rigor of ague is an affection chiefly depending on the nervous system may be proved by many circumstances, but by none more strongly than by the following fact, quoted from a collection of Notices of Russia, published in the *United Service Journal* for January, 1833:—

"In Kasan these fevers are quotidian or tertian, very rarely quartan, and they differ from the agues of other countries in this respect, that the patient experiences scarcely any shivering, but feels a violent twitching in the spine, which is soon followed by excessive heat and violent headache, during which the pulse beats like a hammer. For this fever the Russian physicians resort to no other remedy but bark."

The following description of the Russian province, so fertile in ague, is well worthy of your attention, and I shall make no apology for reading it to you:—

"The summer in this country is further remarkable, inasmuch as from the end of May to the beginning of September no rain falls, and thunderstorms are extremely rare. This phenomenon is doubtless owing to the flatness of the country. For five hundred miles and more around Perm and Kasan, there is not a hill of any consequence, and the whole tract from Kiew to Ural, for the breadth of five hundred miles, may be called a plain, only here and there interrupted by ranges of gentle hills. The extraordinary fertility, especially of the government of Kasan, is occasioned by the inundation of the Wolga, which overflows annually at particular seasons, as regularly as the Nile in Egypt, and converts the whole country, to the distance of ten miles or more from its bed, for five or six weeks, into an immense sea. These inundations of the Wolga, and the other large rivers, Witjatka, the Kama, the Kinel, the Irgis, &c., which discharge themselves into the Wolga, render the countries through which they flow at once lively and fertile. At such seasons you may sail, either for pleasure or upon business, in large two-masted vessels, carrying from six to ten guns, over pastures and cornfields, to the neighbouring towns, which on this account are all situated upon heights; and when the waters have withdrawn into their accustomed channels, the ground forsaken by them is covered, often a yard deep, with a fertilizing mud, in which, during the hot season, all vegetables grow rapidly and vigorously as in a hot-house. At the same time pools are left behind in the low grounds, where the water stagnates for several months, becomes putrid, and generates malignant fevers in the months of July and August, in these otherwise healthy countries. The government of Ufa, particularly, is visited about that time by an intermittent fever, *which attacks the patient every seventh day only*, but is so violent that it generally proves fatal."

If this account be correct, and indeed there can be little doubt of its accuracy, a new species of ague must be established, and to the quotidian, tertian, and quartan must be added a fourth type, whose attacks return every seventh day.

In Ireland we seldom meet with cases of ague with paroxysms so violent as to endanger the patient's life. I lately saw, however, a case of this nature. I was sent for in a great hurry to visit a gentleman residing in the neighbourhood of Donnybrook; he had slept well until four o'clock in the morning, when he was awakened by a general feeling of *malaise*, shortly after which he complained of chilliness, some nausea, and headache. After these symptoms had continued about an hour, his skin became extremely hot, the pain in the head intense, and drowsiness was complained of, which soon ended in perfect coma, with deep snoring and insensibility; in fact, he appeared to be labouring under a violent apoplectic fit. He seemed to derive much advantage from bleeding and other remedies, and to my surprise was perfectly well when I visited him in the evening. The day but one after, at the same hour, the very same symptoms returned, and were removed by the same remedies. I must confess that I could not explain, in a satisfactory manner, the perfect freedom from all cerebral and paralytic symptoms, after two such violent attacks of apoplexy; but when a third attack came on, I then saw that it was a case of the *tertiana soporosa* of nosologists, and I prevented the return of the fits by the immediate exhibition of large doses of sulphate of quina.

Let me now direct your attention to the case of a sailor who has been recently discharged. This boy was one of the crew of a vessel which returned lately from the West Indies, and was exposed to great hardship during his voyage. Boys in his situation suffer a great deal of fatigue and rough treatment; they are the drudges of all on board, and it is impossible to conceive what privations they endure. When the vessels arrive in unhealthy climates, they are generally the first who fall victims to the prevailing malady, and such was the case with this lad, who got yellow fever immediately after his arrival at the West Indies. From this he recovered, but on his way home was attacked with irregular intermittent, which lasted for a considerable time. He had no treatment, and the disease subsided spontaneously, leaving him extremely weak and emaciated. He was, however, obliged to work as usual on his passage, and he arrived in Dublin about three weeks since, debilitated, thin, and with a countenance expressive of long-continued suffering. He had on his admission that peculiar hue of skin which often follows tedious intermittents, and which those who have once seen will always recognize with facility. This colour is to be distinguished from the hue of light jaundice—it is what has been termed a clay colour. In the present instance it was mixed with a faint tinge of jaundice, and on examining the stools we found that they contained scarcely any bile. He had no fever; his pulse was rather slow and regular; he complained of lassitude; his urine was deeply tinged with bile; and his belly tumefied. On examining him, we found that the abdominal tumefaction did not depend on the presence of fluid in the peritoneum: it was produced by enlargement of the liver and spleen, intestinal congestion, and tympanitis.

Here was a case of what has been vulgarly termed ague-cake; that species of congestion and enlargement of the liver and spleen which is apt to accompany the paroxysms of an intermittent, and in some cases to remain after the disease has subsided. You are aware that some persons, during the paroxysm of an intermittent, complain of pain in the right hypochondrium, but more frequently in the left, and on examination the liver or spleen is found increased in size. If you take the trouble of reading the experiments which have been made with the view of illustrating the functions of the liver and spleen, you will have a good idea of the facility with which enlargement of these organs, but particularly of the latter, may take place.

The spleen undergoes very remarkable changes, even in its natural state, during the process of digestion, and there is a great difference between its size when an animal is fasting and its size when an animal has taken food. Indeed, it is surprising how rapidly it will become filled with blood, and how quick the transition is from a state of collapse to a state of congestion. It is easy, therefore, to conceive how the spleen may, during the paroxysms of an intermittent, particularly in the cold or congestive stage, become manifestly enlarged. The increase of size, however, never occurs to such an extent in the liver; unlike the spleen, its magnitude remains nearly the same, and its volume does not vary, like that of the spleen, with the time of day or the period of digestion. It is obvious, therefore, *a priori*, that the spleen should be more frequently the seat of congestion than the liver, and that its enlargement should be more distinct and palpable.

But it is not in the liver or spleen alone that any congestion occurs during an aguish paroxysm; it may take place in any organ; and this, in a practical point of view, is worthy of being borne in mind. Thus, in a case which I attended, the patient got intermittent of a tertian type; during each paroxysm he had some distress about the chest, and slight cough, but these symptoms disappeared during the intervals. As the disease, however, went on, the fits of coughing and dyspnoea increased, and the sulphate of quina failed in arresting the paroxysms. The pulmonary congestion became gradually more marked and permanent, and no longer disappeared during the intervals; finally, inflammation of the lungs took place, and the patient died with extensive hepatization. This happened several years ago, when the old notion of connecting the cold stage of ague with debility was universally prevalent, and before the practice of bleeding for the relief of visceral engorgement had been introduced. Subsequently, the practice of bleeding in the cold stage, as introduced by Dr. Mackintosh, was tried on an extensive scale in the Meath Hospital, and it is a practice which I can strongly recommend in those cases where there is recurring inflammation of some internal organ. It is not a mode of treatment applicable to all cases, and in mild cases, unaccompanied by extensive congestion of any viscus, it is totally unnecessary; but where an important organ is threatened, it is a valuable remedy, and has on some occasions cut short the paroxysms altogether, or rendered them much milder and more manageable.

Sometimes ague is accompanied by symptoms of congestion and inflammation of some internal organ during the paroxysms; and yet, by giving sulphate of quina, you will succeed in arresting the intermittent and the visceral disease at the same time. I recollect the case of a boy who was under treatment here for ague, and who, during the paroxysms, had severe bronchitis with dyspnœa. The cough did not leave him even during the intervals, but it was much milder; I was, however, doubtful whether the case would admit of the exhibition of sulphate of quina, from the violence of the pulmonary symptoms during the fits. I determined, after some time, to try the quina, and I found that it stopped both the intermittent and the bronchitis. It is to be observed, however, that in this case the bronchitis was of a chronic character; and I believe that in all cases of ague, accompanied by visceral derangement, where quina succeeds in curing the disease, the inflammation is either of a trifling description, or is one of a chronic nature. Where the visceral derangement is great, quina will not succeed, and hence it is of great importance, in the treatment of ague, that you should carefully attend to the state of the internal organs.

There are several forms of disease which simulate intermittent in a very remarkable manner; and as this may lead to very dangerous errors, it is necessary on all occasions to make a strict inquiry into the origin and history of the complaint. Some forms of hectic assume the intermittent character, and have been frequently mistaken for ordinary ague. Of this I had lately a very striking instance in the case of a lady who came from the county of Limerick, to consult me for what was stated to be an attack of irregular intermittent. She had been confined in August; had been feverish after her accouchement—the consequence, she believed, of exposure to cold—and got a slight cough. This continued, but without any expectoration, for two or three weeks, and then she was attacked with fever of an intermittent character, and exhibiting a well-marked tertian type. She began to take quina, but this aggravated the cough very much, without having any effect on the paroxysms. Various other remedies were also tried, but their only effect was to render the paroxysms more frequent and irregular. The moment I saw her, I was convinced that she was labouring under some visceral disease. I examined her chest, and found dulness under the right clavicle with tubercular crepitus. Her cough had been dry until she came to Dublin, but now it became suddenly moist, and a distinct gargouillement could be heard. The apparent intermittent was nothing more than phthisical hectic; and Dr. Stokes, who was called in, came to the same conclusion. I recollect having observed something of the same kind in a case which I attended some time ago with Sir Henry Marsh. The patient had well-marked intermittent, and we treated him for it; but the sulphate of quina, and the other remedies which we employed, had only the effect of converting the fever into remittent. On a sudden, the gentleman, without having made any complaint in the side, or anything indicative of derangement of the liver, became suddenly jaundiced, and sank rapidly. On dissection we found seventeen or eighteen small circumscribed abscesses in the substance of the liver. The intermittent hectic here depended on interstitial inflammation of the liver—a disease which is generally of a latent and incurable character.

I need not refer here to certain forms of fever which accompany disease of the brain and of the urinary system, and which are remarkable for their intermittent character. There is, however, one form of anomalous intermittent, of which it may be necessary to say something. I allude to that species of ague which seems to be exclusively confined to females of a nervous habit—at least, I have never met with it in any others. Persons of this description, after an accouchement, or some acute disease, or in consequence of violent mental emotions, will sometimes get into a peculiar state of health, in which they are liable to recurring periodic attacks of fever. Some time since Dr. Stokes called me to see a lady who, shortly after confinement, had got an attack of well-marked tertian. She had, at the regular time, severe rigors, followed by acceleration of pulse, heat of skin, and profuse sweating. When the paroxysm was over, she felt tolerably well, but still there was much excitement of pulse, and the intermissions were anything but perfect. Sulphate of quina had been tried by the accoucheur in attendance, but had failed. On examining the case, I found that the lady was of a decidedly nervous and hysteric habit, and advised the use of nervous and anti-spasmodic medicines. A mixture containing musk, camphor, and ammoniated tincture of valerian was prescribed, and the intermittent symptoms rapidly disappeared.

But to return to the case of this boy. How are we to treat this ague-cake? The disease has not as yet proceeded so far as to produce ascites; but if permitted to run on, it would soon cause effusion into the peritoneal cavity. In a case of this kind, a great deal will depend on whether there is any fever present or not. If there is no remarkable excitement of pulse or heat of skin, general antiphlogistic means will be unnecessary, for any local tenderness or irritation can be relieved by local bleeding. In the case before us, there was a slight degree of tenderness, and we applied leeches once with benefit; but we did not apply them over the abdomen—they were applied to the anus, because it is well known that leeches applied in this situation have a remarkably good effect in removing intestinal congestion, and consequently in relieving hepatic engorgement. Those who have remarked the relief which a flow of blood from piles gives, in cases of hepatic engorgement with dyspepsia, will recognize the value of depletion of this kind, and will imitate the natural mode of relief by art. Hence the use of leeches applied to the anus in cases of intestinal congestion and hepatic or splenic engorgement. There is no necessity here for applying a great number of leeches—three or four every second day will be quite sufficient, and we have found this number answer every necessary purpose. In addition to local bleeding and attention to diet, I ordered this lad to take a few grains of blue pill once a day, not with the intention of affecting his system, but merely with the view of keeping up the free action of the bowels. I continued the mercury only as long as the tenderness of the liver remained; for experience has shown that, in those cases of ague-cake where there is merely enlargement of the liver without tenderness, mercury is a bad remedy.

In cases of this kind, where the stage of active congestion is past—where there is no fever—where the tenderness is removed, and nothing but the increased size of the liver remains—how are you to accomplish a cure? First, by inserting one or two setons over the liver; and, secondly, by the use of iodine and tonics. The use of setons in cases of this description is well known, and needs no comment. I recollect the case of a lady, who, after several attacks of jaundice, got chronic enlargement of the liver. The right lobe of the liver, which was the portion chiefly affected, extended down towards the crest of the ilium, and was excessively indurated. This state had occurred after the patient had used mercury, and had been copiously salivated. Two setons were inserted over the region of the liver, and these produced rapid diminution of the enlargement, and a perfect cure.

With respect to tonics, I may observe that they prove extremely useful in chronic enlargement of the liver and spleen. We are in the habit of using, in this hospital, a combination somewhat similar to the celebrated Bengal spleen-powder; it consists of vegetable and mineral tonics combined with a vegetable purgative—as, for instance, aloes—and we have seen the best results from its use. With respect to iodine, it is a valuable adjuvant in such cases, particularly where the system has been much deranged, and where mercury would be likely to run down the patient. Here iodine gives vigour to the constitution, and tends in a very remarkable manner to promote the absorption of the morbid products on which the enlargement chiefly depends.

Before concluding this lecture, I wish to bring before you some singular facts respecting the liability of the human race to be affected with disease. You are aware that certain affections are peculiar to warm climates, and that these affections prove to an extraordinary degree fatal to whites who may come within the operation of the causes by which they are produced. These causes, more especially as regards Africa, are generally believed to be of malarious origin; just such causes as in colder climates give rise to agues, but in the torrid zone produce a fatal form, not of intermittent, but of remittent fever. How comes it to pass, however, that this peculiar form of fever is almost exclusively confined to Africa, occurring on both its western and eastern coasts, while it is not met with off the shores of South America, where the same physical causes, so far as relate to large swamps with quantities of decaying vegetable and animal matters, apparently exist? Again, in the published accounts of the recent expeditions to Borneo (I especially allude to those of Captain Keppel, in the "Dido," and of Sir Edward Belcher, in the "Samarang"), we hear nothing of the crew being attacked with this fever, although they were constantly exposed to malarious emanations in rivers with swampy banks, lined with mangroves, in which there were low tides, and in the self-same latitude as Cape Coast.

Let us also, for a few moments, contrast the unhealthy condition of intertropical Africa with other portions of the same continent. When I come to speak of the pernicious effects of the western coast on our sailors, I shall, in the case of the "Eclair" steamer, give you a very recent illustration of the deadly character of the emanations by which the coast fever is supposed to be originated. I need scarcely say that the same effect is produced, if possible, in a tenfold degree amongst the *white* dwellers on land in this unhealthy region; but I cannot forbear reading for you a single sentence from Bathurst, on the mortality of one of our settlements. "In 1824, there were 346 European soldiers at Sierra Leone, of these 301 died in the rains; and in 1825, of 1,193 there died 621; and of 108 young men sent to the Isles de Los, to the north of the colony, 62 died." This deadly character of the climate, moreover, affects the inferior classes of animals as well as man. In *Travels in Western Africa in* 1845—6, by John Duncan, it is stated that at Cape Coast Castle agriculture has made little progress, probably owing to the want of horses, *which cannot live more than a few weeks;* but the native breed of cattle is very handsome, though small, and is not subject to disease.

Now there is, probably, not a more salubrious climate in the whole world than Southern Africa. All writers on the recent Kaffir war agree that one of its most remarkable features was, the general good health of the troops, notwithstanding the great exertions and hardships to which they were exposed. And in a very interesting paper recently published by Colonel Napier, entitled *A Few Months in Southern Africa*, I find the following singular and interesting observations:—"The most sudden transitions from heat to cold, and *vice versâ*, is a marked peculiarity of this changeful, though, strange to say, most salubrious climate, in which one may, generally speaking, and with equal impunity, doubt, although the fact has not, as far as I am aware, erto noticed. It must depend on some chemical or ause as yet undiscovered.

absence of any positive knowledge on this subject, are y means by which the almost uniform fatality of the fever may be rendered less destructive to mankind? y efficient method for so doing, I believe, must be or by an investigation into the effects of climate on the race.

a remarkable and curious fact, that man is the only l in whom the identity of species is preserved, while the ns of his physical constitution are so great, that he is ed, aided by the resources reason and experience suggest, habit every latitude, and multiply in every climate; but rto he has seemed unconscious of the value of the gift bestowed by the hand of Nature—and, while history records sad effects of war in diminishing or destroying these eties of the human species, we search its pages in vain for y attempt to preserve or increase them; and yet there can be le doubt that a proper attention to their physical and mental ualities, would soon make known what region of the earth each peculiarly fitted to inhabit, and what duties calculated to perform, in extending the empire of civilization. Let, then, the ulers of nations arrest the hand of destruction—let us have no longer to contemplate such catastrophes as the annihilation of the aboriginal inhabitants of Van Dieman's Land, and let the voice of reason (not to invoke the holy name of religion) stay the structure of the funeral pile, on which may soon be placed the lifeless corpse of that noble member of the human family, the North American Indian.

It has been long known that negroes can withstand the action of deleterious exhalations that are fatal to Europeans. A striking instance of this kind must be fresh in the memories of all. In the expedition to the Niger, undertaken by command of the Government in 1841 and 1842, the mortality from fever was so great as entirely to prevent the execution of the intended design, and one of the iron steamers was saved only by the exertions of the surgeon, who acted as engineer, nearly all the other whites on board having perished. Three steamers were employed in this unfortunate enterprise, and their united crews consisted of

145 white men and 158 blacks; of the former, 130 were attacked with fever in the Niger, and 40 perished; while, of the blacks, only eleven caught fever, and in them the disease assumed a comparatively mild form, and none died. Of the blacks, 133 were entered on the coast of Africa, and consisted of native Africans, chiefly Kroomen, a *littoral* and seafaring tribe, whose intelligence, nautical skill, and fidelity will hereafter render them most available in the hands of some other civilised nations. Of the 133 natives, the greater number had never been on the waters of the Niger before, and yet not one of them sickened; the remaining 25 blacks were entered in England, and consisted of men—some natives of the West Indies, some of the United States of America, and one or two from Nova Scotia; of these, *eleven*, as I before mentioned, contracted fever, and none died, although every one of them had been in England, and absent for several years from tropical climates. This fact, Dr. M'William observes, proves "that the immunity from fever in warm countries, which is enjoyed by the dark races, is to a certain extent destroyed by a temporary residence in another climate." This is quite true, but let us consider it in another point of view. Ten of the twenty-five blacks entered in England were West Indians, and had never visited the Niger, and yet they either escaped altogether, or had but slight fever; and two of the twenty-five were born in cold climates. It appears, therefore, that the black man has a physical conformation which fits him to resist better than the white the deleterious fever of tropical climates.

The same fact is still more strongly proved by the unfortunate results of the fever with which the crew of the "Eclair" steamer were attacked, when stationed on the coast of Africa in 1845. I need not enter into any particular account of these circumstances, so well known to all; it is sufficient for my present purpose to state that, as appears from the official documents, out of forty Kroomen on board, not a single individual was attacked with the fever which proved so fatal to nearly every European on board, until after the vessel arrived in England, when five slight fever cases occurred amongst them, but which are ascribed by Sir William Burnett to their being sent on board the "Worcester," a much *colder* ship than their own.

Again, it is stated by Major Forbes, in his account of an eleven years' residence in Ceylon, that when the English were occupied in constructing the splendid roads which now traverse that island, some of the localities were found so destructive of human life, that even the native Cingalese labourers fell victims to disease in great numbers, and consequently the undertaking must have been abandoned, had it not been found that our Kaffir soldiers, who acted as pioneers, were comparatively exempt from the effects of the noxious exhalations, and by their labours, therefore, the work was perfected in places where heat and moisture, acting on the accumulated vegetable deposit of this extensive wilderness of wood, gave rise to a miasma fatal to the other races employed by the Government.

In a lecture which I delivered before the College of Physicians, in 1844, I entered at length into the history of the different races of mankind, as regards their diffusion over the face of the globe, and for further information on this subject I must refer you to that lecture, which was published in *The Dublin Literary Journal*, of April 1st, 1844. At present, when speaking of ague, I thought that the foregoing observations would prove interesting, as bearing on the malarious origin of disease. But I cannot conclude without expressing my conviction, one which I have arrived at from long consideration given to the subject, that the several original races of mankind were created by the Almighty power, with the view of their peculiar adaptation to the different climates of the globe. And I do not know any more interesting or more benevolent subject of inquiry which could engage your minds, than one into the physical circumstances by which any peculiar variety of the human race is constituted for inhabiting an individual climate.

LECTURE XXVI.

ON THE LAW WHICH REGULATES THE RELAPSE-PERIODS OF AGUE.

I SHALL devote this lecture to the consideration of an interesting subject to which I have lately paid much attention, namely, as to whether there is any law which regulates the relapse-periods of ague.

Having noted with much anxiety and accuracy the course of a quartan ague for twenty-seven months, I constructed a table for the purpose of obtaining a connected view of the number and dates of the fits. This table had been made for some time, before I discovered that it contained *data* which authorize us in concluding that the law regulating the periodicity of agues applies not only to the succession of paroxysms, but is extended to the free intervals between them—in other words, that the same law of periodicity which governs the disease while it occasions fits, continues likewise to preside over its latent movements during the interval when no fit occurs; and thus the true periodic rate is carried on, though, as in a clock from which the striking weight has been removed, the usual signal does not mark the termination of each certain definite portion of time.

This law, now for the first time brought to light, exhibits a new example of the tenacity with which periodicity clings to a disease, when once firmly impressed on it, and recalls to mind a very similar phenomenon observed with respect to the catamenia, which, having been suppressed for many months, not unfrequently reappear on the very day on which the monthly period would have occurred, had no such suppression taken place.

The case I am about to detail possesses likewise several features of practical interest, and serves to show that a very obstinate species of ague, accompanied by various complications, may be perfectly cured by the use of quina alone; and that very large quantities of that powerful medicine may, under such circumstances, be taken not only with impunity but with advantage. A boy of good constitution and eleven years of age had been at a boarding school in Kent, during the spring and summer of 1842, and remained in perfect health all that time. In autumn he was very imprudently allowed to bathe daily in a pond of stagnant water, and he frequently continued in the water for more than an hour. In the November following, feverish symptoms exhibited themselves, and he was several times an inmate of the school infirmary: his disease was considered to be a frequent return of feverish attacks from cold and indigestion; and accordingly he was treated by confinement and low diet, with mercurial and saline purgatives. Notwithstanding these remedies, the disease frequently recurred, nor was its true nature even suspected by the medical attendant. He arrived in Dublin on the evening of the 16th of December, 1842, and the moment I saw him I concluded, from the peculiar tinge of his complexion, that he was affected with ague. He had a slight cough, but in other respects was tolerably well, although fatigued by his journey: he slept well that night. On the 17th of December he made a good breakfast and dinner, but after dinner he sickened: he slept well during the night, and awoke at eight o'clock on the morning of the 18th. He was hot and feverish all day until about eight o'clock in the evening: the paroxysm of ague thus lasting twenty-four hours. He got at four p.m. five grains of sulphate of quina.—19th. Slept all last night, free from fever; sulphate of quina repeated.—20th. No fever: cough much better; third dose of quina.—21st. He slept well during the night; he awoke free from the fever, which, however, returned at eleven o'clock a.m.; the fit lasted eight hours: the quina was repeated.—22nd. The dose of quina was increased to seven and a-half grains, and continued for some days. There was no return of the fever until January the 8th, on which day he had a slight fit. We here remark for the first time, that the paroxysm occurred on the very day on which it would have occurred had it been going on regularly from the 21st of December; for then the days should have been the 24th, 27th, 30th, and 2nd, 5th, and 8th of January; in other words, the periodic time of the disease, while it exhibited no evident paroxysm, was the same as when it did. The quina was resumed on the 8th of January, and seven and a-half grains of it given daily for four days. The disease now disappeared for a time, but on January 21st he had a slight fit; and it is to be remarked that this does not correspond with the day upon which it should have reappeared, viz., the 20th, had its latent periodic time remained the same, as may be seen in the table. To proceed with this particular part of our subject:—paroxysms occurred on the 21st, 24th, and 27th of January, and then ceased, in consequence of the exhibition of quina, until the 10th of March. Now reference to the table will show that had the disease observed the quartan period, from the 27th of January, it would have reappeared on the 10th of March. Fits occurred on the 13th and 16th of March, and then ceased, under the influence of medicine, until the 30th of April, the very day which corresponds with the quartan period, had it gone on regularly from the 16th of March, as may be seen by reference to the table. The fits occurred again on the 3rd and 6th of May, and were then arrested by the use of quina, again to reappear on the 24th of May, the very day the fit was due: but of this more hereafter. We have seen that the fit of the 21st of January was slight, and that on the 24th was severe, commencing at three p.m. The headache was very bad: the fever continued, more or less, to the 25th, and his appetite was not restored until the 26th. On the 27th, about three p.m. another fit, much less severe: scarcely any headache: less heat of skin, nausea, and restlessness: passed a good night, and was perfectly well at breakfast on the 28th.

From the 18th of December to the 2nd of January, he took seventy-five grains of quina; from the 8th to the 12th, thirty grains; and from the 21st to the 30th, sixty grains; total amount, 165 grains. The fit did not return on the 30th of January, and he seemed in every respect perfectly well on that day. Medicine was now discontinued. He had not the slightest indication of disease until Friday, March the 10th. The fit was then, however, so slight that I was doubtful concerning the actual occurrence of a relapse, and therefore did not resume the quina, until a very severe aguish paroxysm on the 13th of March removed all doubt upon the subject. It is particularly worthy of notice that the boy exhibited not the slightest feeling or precursory symptom of indisposition, and had a very healthy colour up to the very beginning of the paroxysm on the 10th. This is not usual, nor did it often happen in the case before us; for, as the disease became more deeply rooted, the return of the fit was invariably preceded, for a few days, by an unhealthy aspect and a pale colour. Still the sudden manner in which the ague fit sometimes commenced is very remarkable, for I have seen this patient sit down to a meal with a good appetite, and he had scarcely half finished when all at once he felt indisposed, every trace of appetite vanished, and the aguish rigor set in. I particularly remarked, too, that there was no derangement whatsoever perceptible in his sleep, urine, alvine evacuations, tongue, or the functions of any other organ, during the twenty-four hours that preceded the relapse of the 10th of March. At later stages of the complaint, this freedom from functional disturbance before the actual fit was not so clear, but, on the contrary, the boy usually felt a little unwell for a day or two before the fit commenced.

These facts show us that ague is at first purely periodic, the health being totally unaffected during the interval between the attacks; but as the disease becomes rooted, as I have said before, in the constitution, the intervals are rendered less purely healthy. On the 14th of March he again began the quina, in daily doses of ten grains. The fit of the 13th had been very severe; that of the 15th was milder, and, as I already stated, the ague then ceased, not to reappear until April the 30th. From the 14th of March until the 17th he took ten grains of quina daily, and then continued the medicine in gradually increasing doses, until ninety grains had on the whole been taken during this month. The paroxysm of the 30th of April was slight but well marked; that of the 3rd of May was sudden, and attended from its commencement with raving and hallucinations, which were very alarming, and lasted for two hours, until the hot fit was established. This fit was not perfectly solved sooner than sixteen hours, and created so much uneasiness in my mind that I resolved, contrary to my previously formed resolution, to give him quina in order to prevent another attack, or at all events diminish its violence, fearing that the disease, if unchecked, might pass into its worst form, the apoplectic or *febris intermittens perniciosa*. Accordingly, on the 4th, 5th, and 6th of May he took forty grains of quina, notwithstanding which he had a fit, as I before mentioned, but slight, and without any cerebral symptoms, on the 6th. The following day he went, by Dr. Stokes' advice, to reside in a cottage most favourably situated over the sea, on the high cliffs of the south side of the hill of Howth; and on the 9th he took, at 2 p.m., a draught containing ten drops of laudanum and twenty of sulphuric ether. He spent his time chiefly in the open air, and his appearance became much more healthy. He remained quite free from the disease, was active, strong, and cheerful, with an excellent appetite and good spirits, and returned to Dublin on the 23rd day of May, having passed seventeen days free from a paroxysm.

On the 24th, at 4 p.m., he had a slight paroxysm, and on the 27th, at the same hour, another, which was well marked but not severe, for he slept well the whole night, and, though he had but little appetite next morning, he was in every other respect quite well. We were led, probably erroneously, to attribute the comparative mildness of this fit to a draught containing camphor mixed with sweet spirits of nitre and seven drops of laudanum, taken at 1 p.m., and repeated at 3 p.m., the latter followed by a cup of hot coffee. On the 28th of May he again went to Howth, and was directed to take an opiate draught on the 30th as before, and to go to bed at three o'clock, and by means of hot tea to try to prevent the fit. Notwithstanding these measures the fit came on at the usual hour, on the 30th of May, and, though not of long continuance, was severe, and its commencement was accompanied by spectral illusions. Dr. Stokes and I now determined to lay aside medicine and try what the pure air of Howth, aided by fine weather and constant outdoor amusement, would do. The event did not justify our expectations, for he had fits on the 2nd, 5th, 8th, 11th, and 16th of June, and these fits came on with great regularity about 3 p.m., some of them slight and interrupting his amusement only for an hour or two, but others severe, and, though not lasting more than six or eight hours, yet attended with headache, nausea, vomiting, and purging, which affections seemed to relieve the head. As he had eight successive fits, and the disease evinced no inclination to subside spontaneously, we resolved again to try the sulphate of quina, and on the 15th gave him five grains twice, on the 16th three times, and on the 17th twice before 10 o'clock, in order to interrupt the fit; on the 18th twice, on the 19th three times, and on the 20th twice; so that he took seventy grains during these six days. The result of this treatment was a milder fit on the 17th, and none on the 20th. Thus the plan of giving no quina had been tried from the 6th of May to the 15th of June, and it is observable that after this uninterrupted series of nine fits, the seventy grains of quina which were required to stop the fits produced only an interval of eleven free days, from the day the medicine was last exhibited, viz., the 20th of June; for on the 2nd of July he had a slight but well-marked shadow of a fit, consisting of paleness, collapse, and some headache, short in continuance, and followed by a scarcely perceptible hot fit. We have already seen that where only two fits had been allowed to occur, forty grains produced a free interval of seventeen, and the advantage therefore of immediately giving quina, and as soon as possible arresting the course of the paroxysm, was so obvious, that on the evening of the 2nd of July I gave him five grains of quina, and twenty grains more were given on the 3rd, 4th, and 5th. Now the good effects of at once arresting the disease in its progress were made very evident, for these twenty-five grains obtained a clear interval, without fever and without medicine, of fifteen days.

On the evening of the 20th he was out boating at Kingstown, and came home chilled, as he said, by the breeze; but as he recovered after tea, and slept very well during the night, we flattered ourselves that it was merely a chill and not the shadow of an ague fit. But on the 23rd he had a decided though not severe fit. He now recommenced quina—five grains on the 23rd, five on the 24th, ten on the 25th, and ten on the 26th, on which day he had a well-marked fit, but not of long continuance, and its commencement was deferred until half-past seven in the evening. On the 27th he took five grains, on the 28th ten grains, and on the 29th ten grains—on that day he had no fit; so that, between the 23rd and 29th, both days inclusive, he had taken fifty-five grains, which quantity produced a free interval of eleven days. This result forms a striking contrast with the former, and proves that twenty-five grains employed immediately on the appearance of the first fit produce a longer interval than fifty-five grains employed after the second fit had been allowed to come on. I now determined to act on the experience thus gained, and give the medicine the moment the disease reappeared. This it did on the 10th of August, when he had a decided fit, which commenced at half-past five, and seemed to have gone off before nine o'clock, for he slept perfectly well, and was free from fever during the night.

well, the medicine was discontinued; but was again resumed on the 31st of August, as he complained of some vertigo, and he took five grains daily until the 8th of September, when he was quite free from ague.

At this period of his treatment I was not aware of the law which governs the return of the ague fit. On looking at the table it was quite evident that the giddiness he complained of on the 21st of August was the precursor of the fit that would have occurred on the 22nd, had not the quina been used; and again, that the giddiness which recurred on the 31st of August was the first shadow of the fit due on that day; and which, no doubt, would have made its appearance in full development on the 3rd of September, had not medicine been used. A knowledge of this law will, therefore, prove of the greatest importance in enabling us to guard against the return of the disease; for, for several weeks after the series of fits has ceased, we can point out to the patient on what days they are liable to reappear; and, consequently, he can upon those days more effectually guard against the occasionally exciting causes of the disease, such as cold, fatigue, &c., and can also more accurately prognosticate his distance from the paroxysm by the greater or lesser degree of health which he feels on the periodic days. As long as they continue as free as the intervening days, the relapse is comparatively distant. But to return to our history. The disease appeared now much less violent than before, for during the two preceding months the fits had been comparatively slight and of short duration, and much more under the control of medicine. On the 8th of September he went to England. By way of precaution I ordered him to continue the quina in the following manner: he was to take five grains for four consecutive days, and then to omit it altogether for the next six days, at the expiration of which the four-day course was to be resumed. Thus twenty grains were given every ten days. This plan of treatment seemed to be attended with much success. For more than two months he had no attack. He gained flesh and improved in looks and spirits, but just as we anticipated the realization of our best hopes, the disease reappeared on the evening of October he 15th, so that from the date of the last attack on the 10th of ugust, sixty-four days had elapsed without an attack, and by rence to the table it appears that the 15th of October was one of the ague days, or rather of the periodic days: so that the disease suppressed during more that nine weeks had yet, for the whole of that time, observed its latent period in the system, and reappeared with wonderful regularity on the ague day. As I before stated, he had a paroxysm on the 15th of October: it was slight, and occurred in the evening: and again tolerably severe ones occurred on the 18th and 21st of October at early periods of the day: but on the 24th the fit was postponed to seven in the evening, and was only a shadow. Between the 16th of October and the 28th he took fifty grains of quina. From the benefit derived from the four-day course with the sixty-day interval, the course of quina was again commenced, and was persevered in for nearly five months, during which he enjoyed excellent health and was free from every symptom of disease, having taken in this period more than 200 grains. However, on the 9th of March, his old enemy once more attacked him. The fit was very severe, occupying part of the 9th and part of the 10th of March. The latter was the day on which it was due, had it returned according to the usual period. This can scarcely be considered an exception to the usual rule, for when the ague returns after a long interval and the fit is severe, occupying the latter half of one day and the beginning of another, we have already seen that the sequel alone will determine from which of the days we are to date: allowing, however, this to be an exception to the general rule, our period becomes once more dislocated, and we set out anew with a periodic time dated from the 9th of March. This periodic time holds true, indicating after an absence of two fits, the fit of the 18th of March, and the next fit, which occurred on the 11th of April, the latter interval consisting of twenty-three days between the two fits. There was a fit on the 14th of April, another on the 17th, and another on the 20th; and none then occurred until the 2nd of July. According to the new periodic time it ought to have occurred on the 1st of July. In this free interval of seventy-two days the periodic time failed to indicate by one day, or rather by half a day, the reaccession of the disease. We must therefore again take a fresh day of departure from the 2nd of July. Another fit occurred on the 5th of July. No fit came on till the 25th of August, that is, there were fifty clear days free from the fit, between these two paroxysms; and the latent periodic time came out true. No fit occurred until the 2nd of November, so that there was now a free interval of sixty-eight days; and the latent periodic time was true to a day. Taking date from the 2nd of November, we have next an interval of forty-one clear days, bringing us to the last fit on the 14th of December, 1844, which coincides with the periodic time. The following is a table of the free intervals which occurred between successive series of fits, showing the respective duration of the intervals which observed the period, and those which did not:—

TABLE A.—FREE INTERVALS BETWEEN SUCCESSIVE SERIES OF FITS.

	INTERVAL.	PERIODIC.	NOT PERIODIC.
		Days Free.	Days Free.
1842	1st	17	
1843	2nd	...	12
	3rd	41	
	4th	44	
	5th	17	
	6th	14	
	7th	17	
	8th	14	
	9th	65	
1844	10th	...	136
	11th	8	
	12th	23	
	13th	...	72
	14th	50	
	15th	68	
	16th	41	

It is worth remarking that all the numbers indicating the free intervals, in which the latent period was observed, consist of multiples of three *plus* two—the reason is obvious.

By this it appears that, in thirteen intervals, the latent periodic time was preserved, so as to indicate truly the day on which the disease reappeared; and that in the remaining three the indication was inaccurate by half a day or more. It is to be noted that two of the failures occurred where the intervals had been very great. We may, therefore, conclude that the law is true of intervals varying from ten to sixty or seventy days; in much longer intervals it is less certain. During the year 1843 twenty-seven fits occurred; in 1844 but eleven, most of which were in the months of March and April, and some of the latter were among the most violent he experienced. The disease, therefore, did not wear itself out, but was cured. He has had no fit during the last year, and has remained free from the disease since the 14th of December, 1844, to the present date.

With respect to the manner in which quina was used, the following observations may be made. At first I gave it in the usual manner, until the particular series of fits ceased; and then persisted in its use for ten days or a fortnight, gradually decreasing the quantity taken. This is the method generally recommended by authors, and it is founded on the notion that it is necessary, where the medicine is given in large doses, not to omit its use abruptly, lest the system should feel the loss of this powerful tonic. My experience in this and other cases leads me to doubt the accuracy of the reasoning upon which this treatment is founded, and I am convinced that, in following this rule, we defeat our own object, by accustoming the constitution to the medicinal effects of the quina at a time when the ague fit is absent. The quina is the proper antagonist of the fit, and while the fits require this medicine, it is borne well by the constitution. On the contrary, when the fits are absent, its curative effects appear to be diminished, and the constitution becomes so accustomed to it that, when the disease again requires it, the medicine no longer exerts its anti-aguish influence. We have an analogous example in the case of mercury, of which moderate quantities, judiciously exhibited, are sufficient to cure the venereal disease, provided the mercury is given when venereal symptoms are present, and only in the quantity necessary to control these symptoms. If it be given by way of prevention, when these symptoms are not present, or in too great quantity when they are, the system in either case becomes saturated with the mineral, but is not protected from the further ravages of the venereal disease. The second mode of treatment which I adopted was calculated to avoid the inconvenience already pointed out. This method consisted of giving the quina for four successive days, and intermitting it for the six following days, thus embracing the interval comprehended in three fits. By these means it was hoped to keep the system sufficiently under the curative influence of quina while we avoided rendering the constitution too familiar with the medicine; the six-day interval preventing it from becoming saturated by the quina. This method of treatment seems to have been eminently successful, and under its influence the disease abated in violence, the frequency of the attacks decreased, and the long interval of 136 days was at last obtained. Finding, however, that though it had broken the violence of the disease it had not extinguished it, I tried another on the third plan, which was to give no quina until a well-marked fit or shadow of a fit occurred, and then at once to use the medicine in large doses, so as to stop the fits as soon as possible. The moment this object was accomplished the medicine was omitted, and was not again given until the paroxysms recurred, when they were similarly treated. This on the whole appears the best method, as it stops the paroxysms speedily, and keeps the medicine in reserve until they reappear. The following table gives a general view of the quantity of quina which this patient took. The quina was prepared by Mr. Donovan, and was of the purest kind.

On the whole, I conceive the readiest method of giving sulphate of quina to be, to add a dose of the powder to about half an ounce of water at the time it is to be taken; it can be readily mixed by stirring with a spoon; and by this means it may be swallowed without the inconvenience which attends the dose when acid is used for the solution of the quina, the action of which might prove injurious to the teeth.

During the summer of 1843, the young gentleman whose case has been related had a very delicate and unhealthy appearance, and while he was under the kind superintendence of Dr. Stokes at Howth, evident tumefaction of the spleen had commenced. He is now robust and strong, and has regained his original healthy complexion.

Table B.

Sulphate of Quina taken during the Years 1842, 1843, and 1844.

1842.						Grains.
Dec.	18.	Two	draughts, each	containing	gr. v.	= x.
,,	20.	Do.	do.	do.	gr. v.	= x.
,,	22.	Do.	do.	do.	gr. viiss.	= xv.
,,	23.	Do.	do.	do.	gr. viiss.	= xv.
,,	26.	Do.	do.	do.	gr. viiss.	= xv.
1843.						
Jan.	1.	Do.	do.	do.	gr. v.	= x.
,,	8.	Do.	do.	do.	gr. viiss.	= xv.
,,	11.	Do.	do.	do.	gr. viiss.	= xv.

						Grains.
1843.						
Jan.	21.	Four draughts, each containing			gr. viiss.	= xxx.
,,	26.	Two	do.	do.	gr. viiss.	= xv.
,,	29.	Four	do.	do.	gr. v.	= xx.
,,	31.	Six	do.	do.	gr. v.	= xxx.
Feb.	5.	Do.	do.	do.	gr. v.	= xxx.
,,	11.	Do.	do.	do.	gr. iv.	= xxiv.
,,	19.	Do.	do.	do.	gr. iii.	= xviii.
March	13.	Six	do.	do.	gr. v.	= xxx.
,,	16.	Do.	do.	do.	gr. v.	= xxx.
,,	18.	Four	do.	do.	gr. v.	= xx.
,,	22.	Six	do.	do.	gr. v.	= xxx.
,,	28.	Do.	do.	do.	gr. v.	= xxx.
April	3.	Do.	do.	do.	gr. iv.	= xxiv.
,,	8.	Do.	do.	do.	gr. iv.	= xxiv.
,,	15.	Do.	do.	do.	gr. iii.	= xviii.
,,	21.	Do.	do.	do.	gr. iii.	= xviii.
May	4.	Do.	do.	do.	gr. v.	= xxx.
,,	6.	Do.	do.	do.	gr. v.	= xxx.
June	15.	Fifteen	do.	do.	gr. v.	= lxxv.
July	2.	Two	do.	do.	gr. v.	= x.
,,	3.	Ten	do.	do.	gr. v.	= l.
,,	23.	Six	do.	do.	gr. x.	= lx.
Aug.	10.	Seven	do.	do.	gr. v.	= xxxv.
,,	21.	Four	do.	do.	gr. v.	= xx.
,,	31.	Three	do.	do.	gr. v.	= xv.
Sept.	5.	Four	do.	do.	gr. v.	= xx.
,,	8.	Eleven	do.	do.	gr. v.	= lv.
Oct.	5.	Four papers of quina, in each			gr. v.	= xx.
,,	17.	Do.	do.	do.	gr. v.	= xx.
,,	18.	Twelve	do.	do.	gr. v.	= lx.
,,	25.	Six	do.	do.	gr. vi.	= xxxvi.
Nov.	2.	Do.	do.	do.	gr. v.	= xxx.
Dec.	1.	Twelve	do.	do.	gr. v.	= lx.
,,	26.	Do.	do.	do.	gr. v.	= lx.
1844.						
Feb.	5.	Twelve	do.	do.	gr. v.	= lx.
,,	28.	Do.	do.	do.	gr. v.	= lx.
March	13.	Do.	do.	do.	gr. v.	= lx.
April	7.	Do.	do.	do.	gr. v.	= lx.
,,	15.	Six	do.	do.	gr. v.	= xxx.
,,	25.	Twelve	do.	do.	gr. v.	= lx.
Aug.	18.	Do.	do.	do.	gr. v.	= lx.
Sept.	4.	Do.	do.	do.	gr. v.	= lx.
Nov.	7.	Do.	do.	do.	gr. v.	= lx.

Amounting in the whole to grs. 1,680, equivalent to *three troy ounces and a half;* of which he took, in the year 1842, grs. 65; in the year 1843, grs. 1,105; in the year 1844, '. 510.

The two tables marked C, represent the fits and intervals during the years 1843 and 1844. D. F., day on which fit occurred. P. T., the periodic time carried on through the free intervals. W. D. marks where the latter falls on the wrong day, *i.e.*, a day on which no fit occurred: a new series here commences in each of the three failures, as I before explained.

TABLE C.—1843.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
1			P.T.					P.T.				
2	P.T.	P.T.				D.F.	D.F.				P.T.	P.T.
3				P.T.	D.F.				P.T.	P.T.		
4			P.T.					P.T.				
5	P.T.	P.T.				D.F.	P.T.				P.T.	P.T.
6				P.T.	D.F.				P.T.	P.T.		
7			P.T.					P.T.				
8	D.F.	P.T.				D.F.	P.T.				P.T.	P.T.
9				P.T.	P.T.				P.T.	P.T.		
10			D.F.					D.F.				
11	P.T.	P.T.				D.F.	P.T.				P.T.	P.T.
12				P.T.	P.T.				P.T.	P.T.		
13			D.F.					P.T.				
14	P.T.	P.T.				D.F.	P.T.				P.T.	P.T.
15				P.T.	P.T.				P.T.	D.F.		
16			D.F.					P.T.				
17	P.T.	P.T.				D.F.	P.T.				P.T.	P.T.
18				P.T.	P.T.				P.T.	D.F.		
19			P.T.					P.T.				
20	W.D.	P.T.				P.T.	D.F.				P.T.	P.T.
21	D.F.			P.T.	P.T.				P.T.	D.F.		
22			P.T.					P.T.				
23		P.T.				P.T.	D.F.				P.T.	P.T.
24	D.F.			P.T.	P.T.				P.T.	D.F.		
25			P.T.					P.T.				
26		P.T.				P.T.	D.F.				P.T.	P.T.
27	D.F.			P.T.	D.F.				P.T.	P.T.		
28			P.T.					P.T.				
29						P.T.	P.T.				P.T.	P.T.
30	P.T.			D.F.	D.F.				P.T.	P.T.		
31			P.T.					P.T.				

TABLE C.—1844.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
1	P. T.		P. T.			P. T.	W. D.	P. T.				
2				P. T.	P. T.		D. F.				D. F.	P. T.
3		P. T.							P. T.	P. T.		
4	P. T.		P. T.			P. T.		P. T.				
5				P. T.	P. T.		D. F.				P. T.	P. T.
6		P. T.							P. T.	P. T.		
7	P. T.		P. T.			P. T.		P. T.				
8				P. T.	P. T.		P. T.				P. T.	P. T.
9		P. T.	D. F.						P. T.	P. T.		
10	P. T.		W. D.			P. T.		P. T.				
11				D. F.	P. T.		P. T.				P. T.	P. T.
12		P. T.	P. T.						P. T.	P. T.		
13	P. T.					P. T.		P. T.				
14				D. F.	P. T.		P. T.				P. T.	D. F.
15		P. T.	P. T.						P. T.	F. T.		
16	P. T.					P. T.		P. T.				
17				D. F.	P. T.		P. T.				P. T.	
18		P. T.	D. F.						P. T.	P. T.		
19	P. T.					P. T.		P. T.				
20				D. F.	P. T.		P. T.				P. T.	
21		P. T.	P. T.						P. T.	P. T.		
22	P. T.					P. T.		P. T.				
23				P. T.	P. T.		P. T.				P. T.	
24		P. T.	P. T.						P. T.	P. T.		
25	P. T.					P. T.		D. F.				
26				P. T.	P. T.		P. T.				P. T.	
27		P. T.	P. T.						P. T.	P. T.		
28	P. T.					P. T.		P. T.				
29				P. T.	P. T.		P. T.				P. T.	
30			P. T.						P. T.	P. T.		
31	P. T.							P. T.				

LECTURE XXVII.

CHOLERA—ITS ORIGIN AND PROGRESS.

While the art of navigation was in its infancy, and communication by land between distant countries unfrequent and insecure, the different races and families of mankind who dwell far asunder on the earth's surface were necessarily unacquainted with the appearance of new, or the existence of remarkable diseases amongst each other, and, consequently, that department of medical science which may with propriety be termed the Geography of Diseases remained uncultivated. Now, however, we approach to a new era, when the means of intercourse between the most distant nations have been so facilitated by the aid of an improved system of navigation, a commerce almost universal, and the daily increasing efficacy of steam power, that we may indulge in the rational hope of seeing the sciences studied after a new method, which will embrace within the range of observation not merely the phenomena occurring in a single district or country, but those which take place over the whole surface of the globe.

Already have the enlightened efforts of our own University, and the genius of one of its professors, prompted the rulers of many kingdoms to join in an alliance destined to establish magnetic observatories in distant regions, so as to make the globe of the earth itself a subject of extended experiment; the philosophers of the new world have combined with those of the old to examine simultaneously meteorological phenomena, and already have the records preserved by observers at sea and land revealed the hitherto mysterious course of storms, and enabled us to map out the extent and direction of the shocks of earthquakes. When we investigate the physical changes which occur in our planet, we are encouraged to repeat and multiply observations, in the hope of discovering general laws whose application will enable us to explain the past and predict the future. But the surface of the earth abounds with beings in whom the creative powers of life display an order of phenomena more complicated and refined than anything existing in unorganized matter. But for this very reason, and on account of this superiority conferred on organized matter through the agency of vitality, each being thus animated is governed by laws which seem incapable of extension even to other living creatures of the same species; and consequently we were led to expect an individuality, an insulation among animals, which will prevent them from exhibiting changes occurring simultaneously among great numbers, and capable of being traced to the operation of general laws.

A closer examination, however, proves that animals and plants are subject to the operation of physical agencies which act upon numbers of individuals at the same time, and thus give rise to great varieties of diseases. Such diseases should be made a special object of study; many of them are, as it were, fixed, stationary, and confined to certain countries and districts. Thus, the goitre, the *tumidum sub Alpibus guttur*, has from the earliest times been endemic in the valley of the Rhone and other parts of Switzerland; modern travellers have observed it in certain parts of South America, and in Kemaon, a subalpine department of Hindostan. Agues, typhus, yellow fever, elephantiasis, beri-beri, Guinea-worm, yaws, Egyptian ophthalmia, are chiefly confined to the inhabitants of certain districts, and, with a host of other complaints, would afford ample materials for the geography of fixed diseases.

On the other hand, there are affections of men and animals which travel from nation to nation, and tribe to tribe; sometimes these moving epidemics progress with such rapidity, that they speedily migrate over the whole earth; at other times they creep along with a slow and stealthy step, but their journey is continued year after year, until they have travelled round the world. The Asiatic cholera affords an example of the latter class, having been twenty years in compassing the earth; while influenza, an example of the former, often traverses the same space in a few months. Thus, the epidemic influenza of 1830—32 existed in Australia, and was afterwards noticed in the northern hemisphere of Moscow, whence in eight months it extended to St. Petersburgh, Warsaw, Frankfort, Paris, London; three months subsequently it appeared in Italy, and shortly afterwards in

Gibraltar. Now it is deserving of attention that this influenza travelled from Moscow to London in eight months, and to the United States of America in seven months more, and, allowing something for the inaccuracy of dates, these data give its rate across the Atlantic only a little speedier than across the Continent.

This forms, as we shall hereafter see, a striking contrast with the progress of cholera from Britain to Quebec, as compared with its march from Moscow to London, and is a fact of considerable weight in arguing whether cholera, like influenza, is propagated by atmospheric influences.

The influenza of 1833 travelled much more rapidly than that of 1832, for, originating in the north-east, there was but a few days' interval between its appearance in Moscow, Odessa, Alexandria, and Paris!

The influenza of 1847, however, appears to have travelled with still greater rapidity. From returns received at the office of the Director General of the Navy, Sir William Burnett, it appears to have prevailed in January and February on the coast of Portugal and south coast of Spain; in January, February, and March, in Newfoundland and New Zealand; in February and March at Valparaiso; in April, on the coast of Syria; July, August, and September, west coast of Africa south of the equator, and in August in Hong Kong!

Influenzas differ from each other not merely as to their rate of travelling, but as to the extent of the earth's surface which they affect. Some, as that of 1782, spread from China all over the inhabited parts of Asia, Europe, and America; while others, as, for instance, the great influenza of 1837, did not reach the new world at all, although it passed the equinoctial line, and was severely felt at the Cape of Good Hope and Australia. These facts are alone sufficient to stimulate our curiosity, and ought to direct the attention of philosophers as well as physicians to the study of endemic and epidemic diseases; nor will their study be destitute of practical benefit, for were the rulers of civilized nations to bring into active operation a number of institutions, which, discharging the functions of *medical observatories*, should observe and record the appearance and symptoms of epidemics, many curious facts relating to their origin and progress would be soon brought to light, and we might then perhaps be enabled to arrive at a knowledge of some general laws respecting their motions. Thus, we could ascertain whether, as has been asserted, influenza always progresses from east to west, never from west to east; whether, originating on one side of the equator, it often passes to the other.

As the means of communication are now-a-days so rapid, it is quite possible to learn the character and the best mode of treating an epidemic disease long before its arrival amongst ourselves; we knew, for instance, the symptoms and best method of treating the influenza of 1837, several weeks before we experienced its shock, and we had for many years been familiar with the symptoms of cholera before we actually witnessed its baneful effects. I have still by me a manuscript copy of a lecture I gave at the Meath Hospital in 1826; in that lecture I actually described, from eastern authors, the symptoms of spasmodic cholera, and prepared the class for its future arrival in Great Britain, a prediction not my own, but derived from that illustrious philosopher and truly excellent man, Dr. Brinkley, then President of the Royal Irish Academy.

The origin and march of the spasmodic cholera will form the subject of the remarks which I mean to lay before you to-day. In India, or more properly speaking, in Hindostan, the spasmodic cholera is not a new disease; partial epidemics of it have occurred at different times since that empire has been familiarly known to the English. These epidemics, however, being almost exclusively confined to the natives, comparatively circumscribed in extent, and limited in duration, did not attract much attention on the part of European writers.

"In 1762 it prevailed very extensively in Upper Hindostan, destroying, according to Le Begue de Presle, thirty thousand natives and eight hundred Europeans. Dr. Paisley, in a letter from Madras in 1774, states that it was often epidemic, especially among the blacks. M. Sonnerat, in the account of his travels in India, between the years 1774 and 1781, mentions that cholera prevailed on the Coromandel coast, and at one period more particularly assumed an epidemic and malignant character. Curtis, in his work on the diseases of India, and Girdleston, in his essay on the spasmodic affections of that country, speaks of an unusual prevalence of the disease during 1781 and 1782. It prevailed in the northern Cicars in the early part of 1781, and in the latter end of March it affected at Gangam a division of Bengal troops, consisting of five thousand men, who were proceeding under the command of Colonel Pears of the artillery to join Sir Eyre Coote's army on the coast. Men previously in perfect health dropped down by dozens, and those even less severely affected were generally dead or past recovery within less than an hour. Above five hundred were admitted into the hospital in one day, and in three days more than half the army were affected.

"In April, 1783, it broke out at Hurdwar, on the Ganges, a spot held peculiarly sacred by the Hindoos, among a crowd of between one and two millions of persons assembled for the purpose of ablution in the holy stream. It is the custom of the pilgrims to repair to the bed of the river, where they pass the night, with little, if any shelter. Very soon after the commencement of the ceremonies, the cholera attacked the pilgrims, and in less than eight days is supposed to have cut off twenty thousand of them. The disease was, however, on this occasion so confined in its influence, as not to reach the village of Jawalpore, only seven miles distant."*

In Europe no such disease as spasmodic cholera had been known; this assertion, though opposed to some authorities, may be considered as well founded, and indeed I have no doubt of its accuracy. With us spasmodic cholera is an imported disease; in Hindostan a resident epidemic. What causes combined to convert a malady habitually confined to the Indian peninsula, into a disease which overshadowed the earth, sparing no nation nor language, it would be useless to inquire; the subject is buried in profound obscurity: in the meantime, let us hope that it will not prove a permanent addition to the nosology of every country, and that it will soon return within its former limits. It was in the spring of 1817 that the cholera of India assumed a new and more powerful character; it was then it became endowed with properties that rendered its extension steadily progressive over the earth, in spite of all the obstacles interposed by diversity of soil or climate. The disease first assumed the migratory and epidemic form in districts bordering on the Ganges and some of its tributary rivers, at a distance varying from 80 to 150 miles from Calcutta. This took place in the spring and summer, but the date of its commencement is usually referred to the period of its outbreak at Jessore, on the 19th of August, 1817, where the epidemic was first immediately observed and described by Dr. Tytler, who erroneously attributed it to the use of bad rice. Jessore is situated in Gangetic Delta, about 100 miles north-east of Calcutta. The cholera was now observed in general to follow the course of the rivers and soon arrived at Calcutta, where it commenced its ravages in September, 1817, and continued to rage during nearly the whole of 1818.

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* *American Cholera Gazette*, p. 3.

"By the latter end of September the disease was prevailing throughout the whole province of Bengal, from the most easterly limits of Purnea, Dinajpore, and Silhet, to the extreme borders of Balasore and Cuttack; and from the mouth of the Ganges nearly to the confluence of that river with the Jumna, a space of upwards of four hundred miles. Few places escaped the invasion, and the cities of Dacca and Patna, the towns of Balasore, Burrisaul, Rungpore, and Malda suffered severely. The large and populous city of Moorshedabad, which from extent and local position was apparently favourably circumstanced for the attacks of the epidemic, it is remarkable, escaped with comparatively little loss, while all around was severely scourged.

"During the autumn of 1817 the disease extended itself to Mozufferpore and beyond the precincts of Bengal, and appeared at Chuprah, and at the cantonment of Ghazeepore: its attacks in these places were, however, confined to the towns themselves, or villages in their immediate vicinity; the principal portion of the adjoining country at this period entirely escaping the disease. Early in November it attacked the grand army, then stationed at Bundelcund, a portion of the Allahabad province. This army had been assembled in anticipation of a war with the Pindarees, and the centre division, consisting of ten thousand fighting men and eighty thousand camp followers, was encamped on the banks of the Sinde, under the immediate command of the Marquis of Hastings. Here the cholera exercised its most destructive power. It is uncertain whether it made its first approaches on the sixth, seventh, or eighth of the month. After creeping about, however, in its wonted insidious manner for several days among the camp followers, it seemed all at once to have gained vigour, and burst forth with irresistible violence in every direction, extending through the whole camp before the 14th of the month. Old and young, European and native, fighting men and camp followers, were alike subject to its attacks, and all equally sunk in a few hours under its pestilential influence. It was a common occurrence for sentries to be suddenly seized at their posts, and having been carried in, to have two or three successors before the two hours' duty was performed. Many of the sick died before reaching the hospitals; and even their comrades, whilst bearing them from out-posts to medical aid, sank themselves, suddenly seized with the disorder. The mortality at length became so great that there was neither time nor hands to carry off the bodies, which were thrown into the neighbouring ravines, or hastily committed to the earth on the spots where they expired, and even round the walls of the officers' tents. In the five days included between the 15th and 20th of November, the number of deaths amounted to five thousand. The natives, thinking their only safety lay in flight, deserted in great numbers; and the highways and fields for many miles round were strewed with the bodies of those who had left the camp with the disease upon them, and speedily sank under its exhausting influence. The camp being now cumbered with the sick, the Marquis of Hastings determined to seek a purer air for the recovery of his sick. Although every means was put in requisition for their removal, a part was necessarily left behind. 'And as many who left the carts, pressed by the sudden calls of the disease, were unable to rise again, and hundreds dropped down during every subsequent day's advance, and covered the roads with dead and dying, the ground of encampment and line of march presented the appearance of a field of battle, and of the track of an army retreating under every circumstance of discomfiture and distress.'* The exact mortality could not be ascertained, but it appears that of the fighting men, seven hundred and sixty-four fell victims; and it was estimated that about eight thousand camp followers, or one-tenth of the whole, were cut off. On arriving at the high and dry banks of the Betwah at Erich, the army soon got rid of the pestilence, and met with returning health.

"During December the disease appears to have everywhere abated, and in January of 1818, to have become nearly extinct. Towards the latter end of February it however revived with great force, and before the close of the year the whole peninsula of

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* *Bengal Report*, pp. 12—15.

India, from Silhet on the east to Bombay on the west, and from Deyrah on the north to Cape Comorin on the south, had suffered from its ravages."*

The ravages of the disease were much facilitated and increased by the superstition of the people, who, in obedience to the Brahmins, collected in prodigious multitudes on pilgrimages to certain favourite shrines, where they prayed for the cessation of what they were taught to believe the cause of the epidemic, viz., a violent and protracted battle between the god and goddess answerable for the tranquillity and happiness of that part of the world.

During the year 1818 the cholera pursued a threefold route. First, ascending the Ganges and the Jumna, it reached the northern provinces of Hindostan, but was there checked in its progress for several years by the Nepaulese mountains, and finally entirely arrested by the Himalaya range. This is easily accounted for by the thinness of the population of these situations, and the little intercourse which takes place between the mountainous districts and lower regions. Cholera did not in India attain to an elevation beyond six thousand feet above the level of the sea; in June, 1818, it had reached the range of mountains between Nepaul and Hindostan; it was at Schaurapoor, many hundred miles to the north-west, in October; and before the end of the year had ravaged nearly all the numerous cities and villages situated in the vast tract of country watered by the Ganges, the Jumna, and their tributaries. This was one of the most thickly inhabited parts of India, and the destruction of life was awful.

The second route was southward along the coast from one seaport to another, until it reached Madras on the 20th of October, 1818. Here at the very onset of the disease, twenty medical men were attacked, of whom thirteen died.

Sadras, Pondicherry, and the whole Carnatic were affected during the succeeding year; but even in December, 1818, it had reached Jaffnapatam, the most northern town of Ceylon, having passed thither after travelling along the whole coast of Coromandel. On the 10th of January, 1819, it broke out in Colombo, and produced dreadful devastation on the western coast of Ceylon; the disease became exhausted there, but at the same moment burst forth with renewed vigour in Candi, the capital, 2,500 feet above the level of the sea. The cholera did not arrive at the east coast of Ceylon until 1820, when it appeared imported, as was said, into Trincomalee by the flag-ship "Leander." The epidemic was brought to the western coast of the Indian peninsula, partly by sea round Cape Comorin, and partly by the great overland lines of communication which connect the Presidency of Bombay with the Presidencies of Madras and Bengal.

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* *American Cholera Gazette*, p. 19.

It first showed itself at Bombay on the 9th of August, 1820, and in that Presidency carried off 150,000 persons.

The third route of cholera in India I have already referred to: it was across the peninsula from the east coast to the west. It came by Nagpoor, Ellishpoor, Aurungabad, Siroor, and Poonah to the Bombay coast, and was introduced either by troops or travellers.

From Ceylon the disease went to the Mauritius and the Isle of France, whither it was said to have been imported on the 29th of October, 1819. The distance thus traversed at one spring was three thousand miles. Thence it soon passed to the Isle of Bourbon; and in the year 1820 to the east coast of Africa at Zanguebar. It is remarkable that it never reached the Cape of Good Hope, where the strictest quarantine was observed.

The following are the dates of its arrival in the subjoined places:—Burmese Empire generally; Aracan, Ava, 1819; Malacca, 1818; Sumatra, 1819; Java, Batavia (fearful), 1821; Madura, Macassar, after Batavia. Amboyna, in Moluccas, 1823. Amboyna was the farthest south-easterly point it attained to.

The disease visited Borneo and Celebes; and in 1820 broke out with extraordinary violence in the Philippine Islands, principally at Manilla, where the natives, misled by the idea that they were the victims of poison administered by the Europeans and Chinese, rose *en masse*, and were not put down until 15,000 lives had been sacrificed in the contest. Similar manifestations of feeling led to some loss of life even in Petersburgh and Paris, when cholera reached these cities. The same suspicions agitated the inhabitants of Europe during the ravages of the black death in the fourteenth century, when the Jews were slain in great numbers as authors of the plague. In Great Britain I am not aware that any such insane popular ideas were manifested when cholera appeared. In Ireland nothing of the sort was displayed;

and barbarous, cruel, and uneducated as we are said to be, the visitation was in no country met with greater intrepidity and resignation than in our native land. When a city or town was attacked in Ireland, we never witnessed the flight of the better classes; there was neither migration into the country nor desertion of their poorer fellow-citizens. No; I record the fact with pride; every one remained, every one was ready to do his duty and abide in his place until the plague was stayed. In Dublin, and generally throughout Ireland, the members of the medical profession, and the public at large, believed the malady to be contagious, and yet the sick were never abandoned by their friends in private houses, nor in the least neglected in the hospitals.

In 1819 the cholera appeared in Siam, Bankok, Tonkin, Cochin China, and caused immense loss of life in Cambodia. In 1810 it arrived at Macao, and was said to have been imported by some ships; thence it travelled to Canton in China, and, coming to Nanking in 1820, penetrated as far as Peking in 1821. In China the disease proved particularly fatal, on account of the denseness of the population of the Celestial Empire.

So far we have followed the cholera chiefly southward and eastward in the first instance, but afterwards far to the north; in this part of its course it passed 10° to the south of the line, and then, resuming a northerly direction, went on to Peking, in latitude 40° north. Even this portion of its progress leads forcibly to the conclusion, that it followed the track of commerce, whether by land or sea, and was not dependent for propagation on mere local influence, or climate. *There is a popular idea current, that its course was westward; such was the case in Europe, but in most of Asia it was eastward.*

I have already said that the Himalaya range opposed the progress of the disease northward from Hindostan, and that the highest altitude it attained to was six thousand feet. With respect to this latter point, I learned from my friend Captain Meredith, of the 13th Regiment, that it broke out in the medical depot at Landour in 1838, for the first time at a height of eight thousand feet above the level of the sea. It is worthy of remark that cholera did not come to New Holland, although it was in several islands, as Borneo and Celebes, to the north of Australia; but it is to be noted that there is little or no communication between them and the settled portions of New Holland.

Let us now trace its course westward from Hindostan. The general belief in Persia is, that the disease was brought in ships from Bombay to Mascate, Bender-abassi and Bassorah, in which places it appeared nearly at the same period, in spring, 1821.

From Bassorah and Bender-abassi the epidemic spread, in a well-defined and marked manner, along the rivers and routes most frequented by commercial travellers.

Thus from Bassorah it crept up the Euphrates and Tigris; and in August, 1821, was at Bagdad, where it carried off great numbers of the Persian army then besieging that city. Along the Euphrates it proceeded to the ruins of Babylon, and by the great route of the caravans across the Desert it arrived at Aleppo. Here it did not commit great ravages, and ceased in the following December; but afterwards extended to different towns in Asia Minor, as Mosul, Merdin, Darbeker. At Alexandretta, situated on the Gulf of Scanderoon, it did not arrive until 1823. It is strange that cholera did not continue very long in Asia Minor or Syria, and did not at that period penetrate into Egypt.

From Bender-abassi in Persia cholera travelled along the great mercantile road to Shiaz in August, 1821; and thence to Yezd, where it appeared towards the end of September; but on the approach of winter lay dormant until spring, 1822, when it again showed itself, and spread north-westward, committing the greatest ravages in every town and village situated on the great caravan road. Tauris, Korbia, Ardabil, and the provinces of Kalkhai, Masinderan, and Gilan (on the Caspian), were soon infected. In most of these places it seemed to cease for a time, but reappeared in the middle of 1823; and travelling along the Persian seaports of the Caspian, it reached the province of Shirwan, then lately ceded to Russia. Here it ascended the river Cur, and progressed along the high-roads to the fortress of Buku; and on the 21st September, 1823, Astrachan was attacked. In June, 1823, cholera showed itself in the neighbourhood of Laodicea and Antioch (modern names), and then spread in two directions along the coast of the Mediterranean, but disappeared again both there and on the coast of the Caspian Sea.

On the whole, then, the epidemic, from its commencement in 1817 till the end of 1823, had travelled over ninety degrees of longitude and sixty-six degrees of latitude, viz., from the Philippine Islands to the coast of Asia Minor, and from the island of Bourbon to Astrachan and to the Caspian Sea.

It is very remarkable that cholera did not come to Europe by way of Asia Minor; this circumstance may perhaps be explained by the accident of its not having infected Smyrna, the chief seaport of communication between Asia Minor and Europe. Had Egypt likewise been then attacked by cholera, it is doubtful whether Europe would have been so long spared. Be this as it may, from the end of 1823 until its outbreak at Orenbourgh in 1829, cholera seemed to halt on the very confines of Europe, so that we may consider the years from 1817 to 1823 as constituting the first period in the progress of this epidemic.

But although the cholera ceased to attract much attention in Europe during the interval which elapsed between 1823 and 1829, yet we are not on that account to conclude that it lay entirely dormant, for we find it continued its ravages in its original seat, India, and extended itself from Asia Minor, Persia, and China through the vast regions of Tartary and Chinese Tartary.

The thinness of the population in these half desert regions may be the reason why the progress of the disease through them was at once so uncertain and so slow. The want of frequent communication between even neighbouring districts may have baffled for a time the march of the pestilence, and may have occasioned its remarkably slow progress towards the Russian frontier. Certain it is that this march in Persia, Tartary, Mongolia, and Thibet, countries absolutely destitute of regular roads, formed a striking contrast with its rapid transmission through more populous and highly cultivated countries, or its still quicker passage from one maritime nation to another, when connected by a constant trade, as from Germany to England, from England to Canada, and from the East Indies to the Isle of France. In the latter cases the epidemic sprung from one country to another; *but it is remarkable that it never traversed the ocean at a rate exceeding that of ships.*

We next come to the second period of the history of cholera, when it broke out at Orenbourgh in August, 1829, where it raged with great violence, spreading throughout the whole of that Russian province; while the disease, after long lingering in the north of Persia, assumed, in 1829, an increased energy in that kingdom, from whose northern portions it spread along the western coast of the Caspian, arriving at Salian, and the province of Shirwan in June, 1830, and thus spreading to Baku, Kuba, and Sheki, in Chomath Talisch, and in the district Elizabethpol. From this the epidemic pursued a twofold route; the one following the Kura upwards, led to Tiflis, where the mortality reached five thousand: and thence to the Black Sea and the Caspian, until it a second time reached Astrachan, and proved much more fatal in that city than in 1823—now counting more than eight thousand victims.

From Astrachan the progress of the cholera up the Wolga, or Volga, was very remarkable, as it spread from town to town on that river, in the direct route of intercourse and traffic. I may here remark that whenever cholera travels up the highest mountain passes, as in India, or traverses the ocean, as to the Isle of Bourbon, or accompanies the caravan across the desert, as when it arrived at Mecca and Medina, or when it ascends rivers, making the towns on its banks the successive stages of journey; in all such cases, cholera, I say, seems regulated by no common physical circumstance, *except human traffic and human intercourse;* for in other things these lines or routes differ remarkably from each other. But, to follow its ascent of the Volga: in 1830, in August, it came to Saratow, and shortly after to Kazan, Nijni-Novgorod, Kostroma, Jarislaw, and so on to the circle Tischwin, in the government Novgorod, where it was only 250 versts distant from Petersburgh, and where it attained for that year to its highest northern limit.

From the country between the Caspian and Black Sea it spread through the Caucasus to the Don, which it ascended, while it coasted the Black Sea to Cherson and Odessa, in September and October, 1830.

The stream of cholera which entered Russia from the northern provinces of Persia, as it may be seen from the foregoing account, soon formed a junction with that which flowed from Tartary through Orenbourgh.

In the middle of September, 1830, the disease appeared in the government of Moscow, and on the 20th of September in the capital itself, and did not cease until the following March. In Moscow a severe frost and snow set in towards the end of November, without in the least diminishing the diffusion or the intensity of cholera. Its unabated continuance throughout the whole of a Moscow winter is a fact worthy of attention. In Moscow, according to Jähnichen, there sickened between thirty and forty per cent. of the persons who had hospital duty to perform, including physicians, nurses, &c., while of the whole population not more than three per cent. took the disease. In Dublin, likewise, great numbers of the hospital attendants were affected, and many died; still more were saved by the timely exhibition of remedies. It is not quite correct to affirm that cholera ceased in Moscow in March, for in the autumn of 1831 more than one thousand cases occurred.

During the winter and spring, 1830—1, cholera spread far to the west and south, viz., to Kaluga, Tula, Pultawa, Kiew, Podolia, Bessarabia, Bulgaria, and Silistria, and through the river provinces of the Dnieper, the Bug, and the Dniester.

In the more northern and eastern governments the disease had ceased, while it continued, though in a milder form, in the provinces Nicolajew, Crakow, Tauris, and among the Cossacks of the Black Sea. Petersburgh a second time remained untouched, although the disease had arrived at Tischwin, within one hundred miles of it, an immunity to be attributed to the strict precautionary measures adopted, and the *cordon sanitaire* drawn around the capital for the protection of its inhabitants, but not of its emperor, Nicholas, who, it is but just to add, had gone to Moscow the moment he had ascertained the existence of cholera in that city, in order to exert himself in alleviating the sufferings of his subjects. The fear of infection proved no obstacle to the czar, who zealously performed his duty on that trying occasion.

The war in Poland accelerated the invasion of cholera into that unhappy country, into which the Russian army commenced its march on the 5th of February, 1831, in three columns, of which many battalions came from infected provinces. Thus the governments of Volhynia, Grodno, and Wilna were extensively under the influence of disease in the spring of 1831. During this campaign the Russian army lost great numbers by cholera, and Marshal Diebitch himself died at Pultusk, on the 10th of June, 1831, after a few hours illness—a circumstance which gave rise to the unfounded rumour that he was poisoned; the details of his illness have been published by an eye-witness, Dr. Koch, of the Prussian service. In Warsaw the disease appeared on the 14th of April, after the battle of Iganie, where the Poles took many prisoners, who were brought to Warsaw. In Poland the disease advanced and retreated with the infected armies in a striking and remarkable manner. Westwards and southwards from Warsaw it spread rather slowly towards the Prussian confines, arriving on the 23rd July at Kozieglow, a little town nine miles south of Czenstochowa, and but two German miles from the frontier of Silesia.

Northwards the disease had spread in March and April, through Lithuania, to the seaports of the Baltic, particularly Riga. From Riga the cholera advanced through Courland and Liefland (Livonia).

Petersburgh was now threatened on every side, for the disease broke out with renewed violence in the European provinces formerly affected, while most of those which had hitherto escaped suffered in their turn. Under these circumstances the metropolis, considering the great quantities of goods and passengers who arrive by water-carriage from the interior of the country, could not be expected to remain long exempted, although all possible precautions, short of entirely preventing communication with the country, were adopted; accordingly cholera appeared in Petersburgh in July, 1831. Very serious disturbances arose in the Russian metropolis among the lower orders, who considered the pestilence as artificially produced for their destruction by secret friends of struggling Poland. These troubles were only appeased by the presence of the emperor, but not before the mob had destroyed the cholera hospital, and murdered one of the physicians. During this epidemic seventeen medical men died in Petersburgh, and a great many others were attacked, some slightly, some severely. The hospital nurses, porters, and attendants suffered in a very large proportion, as did a great number of the mob engaged in sacking the cholera hospital. Cholera had already invaded several of the most northern provinces of Russia, and had arrived at Archangel in May, 1831. Archangel is the most northern emporium of commerce in the world, and is the highest latitude attained to by cholera, which in a population of 19,000, destroyed more than 1,200. In the beginning of August cholera arrived at Helsingfor; and of September, at Abo in Finland. After this, Aland and the neighbouring islands were affected, and so it passed into Sweden. Dantzic, 30th May, 1831; Elbing, 11th July; consequently eleven weeks after its appearance in Dantzic: but there was an interruption, or rather a great diminution, of the intercourse between these towns. From Dantzic the disease radiated in every direction throughout the neighbouring province. Thorn, 21st July, 1831; Konitz, 22nd August; Memel, 27th July; Königsberg, 22nd July—here a formidable cholera insurrection took place. Stettin, 25th August, 1831; Berlin, 30th August; Frankfort on Oder, end of September; Magdeburgh, 3rd October.

From Magdeburgh the disease spread extensively upwards, along the course of the Elbe. Halle, 20th December, 1831; Merseburgh, 1st January, 1832; Breslau, 23rd September, 1831. In the first months of 1832 cholera had nearly disappeared from the German provinces of Prussia.—Deaths, 31,000. Aamburg, 7th October, 1831. Mecklenburg, 7th October, 1831. Mecklenburg-Schwerin took most extraordinary precautions, and escaped.

Saxony, though Prussia and Austria on either side of it were severely visited, adopted strict measures of precaution, and escaped; the cholera was neither at Leipzic nor Dresden! Hanover also escaped, with the exception of Lüneburgh, 22nd October, 1831. Sachsen-Weimar, Gotha, Anhalt, Hessia, Brunswick, and some other small principalities all escaped, and apparently by the same means, viz., non-intercourse with infected places.

In some Saxon villages, as Cosing and Edderitz, the disease broke out but did not spread, apparently in consequence of the measures of precaution instantly put in force by the authorities.

Austria suffered most severely; Brody (Gallicia), 5th May, 1831; Limberg, 22nd May; all over Gallicia in 1831. Died, 97,770.

Cracow seems to have been infected, not from Poland, but from Gallicia.

Beginning of July, 1831, cholera began in Hungary. In beginning of June, 1831, much popular violence. Spread very rapidly. Pesth, middle of July; Presburgh, 9th September, 1831.

In Hungary cholera had ceased as an epidemic by the beginning of April, 1832, having proved fatal to at least 240,000 persons. Vienna, 15th August, 1831; Prague, 28th November, 1831.

Bohemia was widely affected; but the disease did not spread from Vienna far either to the south or west, and accordingly Carinthia, Stiermark, and the Tyrol escaped, all being protected by the strictest precautionary measures.

It is worthy of being noted that cholera remained, as it were, stationary and in a suppressed form during the winter of 1831 and 1832, in Hungary, Bohemia, and Germany. It did not spread into Saxony, Mecklenburg, Bavaria, and scarcely into Hanover, although these bordered on infected states, an immunity not to be accounted for by the existence of any natural boundaries, as mountains or rivers, for the limits are mostly conventional between the infected principalities and those which escaped; many have, therefore, attributed their escape to the precautionary measures taken. It is strange that Leipzic was spared, while Halle suffered so long and so severely; the situation of the former city appearing to be much more favourable to the development of *miasma* than that of the latter.

Moldavia, in spring of 1831. In Jassy the deaths exceeded 6,000 out of a population of 27,000. The disease began in June; and no doubt its diffusion was favoured by the unhealthy position of the town, and the condensation of a wretched population, chiefly Jews and Gipsies, in its filthy narrow streets. All the medical men, except three, perished with most of their families. Bucharest, July, 1831; Bulgaria, July, 1831; Constantinople, July, 1831; Adrianople, Gallipoli, Philippopili, September, 1831.

It is to be noted that plague broke out in Constantinople at the same time with cholera; but while the latter epidemic ceased towards the end of September, the former continued for several months longer. Cholera now a second time invaded Asia Minor, and, simultaneously with plague, caused great devastations. Corfu, October, 1831; Monastori, in Greece, November, 1831.

The destruction of religious pilgrims at Mecca was appalling. The place resembled a field of battle, so great were the numbers of the unburied dead; and at last even the fanaticism of Mussulmans was forced to yield, and the survivors sought safety in a hasty and tumultuous flight. Three-fourths of the pilgrims are calculated to have perished during the three days they were densely crowded together at Mecca; and of the fugitives 10,000

This book is the prop
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fell victims on their journey. The Pasha of Egypt now repeated the precautions so successful in 1823, but this time they were taken in vain, because, as is supposed by many, they were not resorted to sufficiently soon; be this as it may, cholera broke out first at the two quarantine stations, where the pilgrims from Arabia were detained; and in the middle of August, 1831, it appeared in Cairo and Damietta, and towards the end of the month in Alexandria. Egypt lost on the whole 150,000. The cholera ascended the Nile, and was at Luxor, the site of ancient Thebes, by the end of September.

We next find the cholera visiting England; it arrived about the 4th of November at Sunderland, a seaport directly opposite to, and commercially connected with Hamburgh. The cholera spread through many towns in the north of England, but did not anywhere rage with very destructive violence, a circumstance attributable perhaps to the more complete separation of families in Great Britain, as compared with our continental neighbours. The existence of the disease was announced on the 27th of January, 1832, in Edinburgh, and on the 10th of February in London. The ravages of the cholera in the metropolis were comparatively insignificant, its victims during the whole epidemic not exceeding 1,500.

It is exceedingly remarkable, how many of the great towns of England either escaped infection altogether, or were visited by only a trifling outbreak of the disease.* Up to the 24th of June, 1832 (that is, during a period of about eight months since its first appearance in Sunderland), the total number of cases throughout Great Britain, inclusive of London, amounted to only 14,796, and the deaths to 5,432.† The disease, it is true, continued in many places to linger long after the above date, and reappeared as an epidemic in some places in 1833 and 1834; but still we are quite warranted in concluding that on the whole, in Great Britain and Ireland the cholera did not count 30,000 victims. In Ireland, particularly in Dublin and Sligo, the mortality was much greater than in England—an occurrence which may, perhaps, be accounted for by the bad diet of the Irish lower classes, and the crowded state of their dwellings, it being well known that in the worst quarters of the city many families reside on the same floor, and frequently more than one in the same room. "In London," says Dr. Elliotson,* "the greater part of the people are well fed, better fed than in any other part of the world; they eat more meat, and the flesh is of such quality as is scarcely to be found in any other country. Besides which, they are better clothed and more comfortable; and instead of trashy wines they have good sound ale and porter, and malt liquor of all kinds. But in Paris the water the inhabitants drink is very bad; the people are crowded together, I know not how many families in a house, with little ventilation. The streets are narrow, the houses dirty; and the population live upon what Englishmen consider trash, not roast beef and mutton, but all sorts of dishes made up of bread and vegetables, with a little meat boiled in water to colour it or give it a flavour; and drink not good beer, but thin wine."

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* Cholera commenced in Liverpool on the 12th of May, and in the meantime had visited Hull, York, Leeds, Manchester, and Warrington.

† *Medical Gazette*, vol. x. p. 400.

Certain it is, no matter how we may attempt to account for it, that cholera was much more destructive in Paris than in London, 385 deaths having occurred in one day, 8th April, 1832, in the former city. Nothing has puzzled or perplexed the continental physicians more than the comparative immunity from cholera enjoyed by England, notwithstanding their predictions that *there* its ravages would attain to a *maximum*, for they contended that in the English towns many circumstances would contribute to render the disease more liable to spread, as, for example, their very dense population, the extreme poverty and bad diet of the lower orders, and the damp, foggy nature of the climate. Now, I believe that the reproaches made by foreigners respecting the extreme penury of the lower orders in England are not well founded, at least comparatively speaking, and with reference to the same class of persons in the continental cities; and I am persuaded that in English cities the diet of the poor is superior to that of the continental poor. Indeed, foreign physicians have tried their ingenuity to account for the slightness of the ravages of cholera in Great Britain, some attributing the immunity to tea, some to the quantity of meat we consume, and some to the vapours arising from our numerous coal fires; and each of these hypotheses have been met by objections, for the Chinese, the most national tea-drinkers in the world, were wofully scourged by cholera; and the city of Halle, in Germany, the most devastated town of that country, uses nothing but coal for firing. It is to the more substantial nature of English fare, to the superior cleanliness of that nation, and to their living in families separated from each other, that we must attribute their comparative exemption from cholera, an exemption the more remarkable, when we consider that in England commercial and private travelling between town and town is more rapid, and ten times more frequent than on the Continent.

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* *Medical Gazette*, vol. xii. p. 628.

Cholera first appeared in Paris on the 24th of March, 1832, and it has been argued by those who deny the contagious nature of cholera, and its importation from abroad, that in France it broke out suddenly, not on the confines, but in the heart of the kingdom, and consequently that it must have arisen spontaneously in the metropolis. Before we attach much weight to this argument, we must have very strong proofs that the facts are as above stated. Now, it is very remarkable that cholera was officially announced to exist at Calais only eight days after it appeared at Paris: and when we recollect how unwilling the authorities in all seaports of hitherto unaffected nations have invariably been to acknowledge the existence of cholera, it is not by any means improbable that cholera may have existed in Calais before it broke out in Paris—a supposition confirmed by the report of Arnaud, Moribaud, and Gendrin, who witnessed in Calais, towards the end of 1831, many very violent cases of cholera resembling the Asiatic; nay, even after the cholera had manifestly appeared in Calais, many persisted in declaring that its victims died of common enteritis.

We see cholera introduced probably from England to Calais, and immediately after to Paris, from which it radiated in all directions by slow and varying stages all over the kingdom. The position of Paris, and its daily communication with England, rendered it almost the first prey of the disease in France. Once there, the cholera moved along the different lines of communication in every direction, its route not governed by any of the laws observed by epidemics depending on atmospheric changes; and its gradual progress from Paris, as a centre, towards all parts of the circumference of France, presenting a course obviously opposed to that of such epidemics.

From England cholera soon spread to Ireland; the following dates of its arrival were communicated by Dr. Barker, whose official situation in the Board of Health gave him the best opportunity of ascertaining the progress of the disease :—

Places.	Dates of Outbreak of Cholera.
Dublin	22nd March, 1832.
Arklow	8th April, ,,
Banbridge	9th April, ,,
Cork	12th April, ,,
Ramelton, County Donegal	12th April, ,,
Naas	13th April, ,,
Belfast	14th April, ,,
Warrenpoint	17th April, ,,
Stranorlar, County Donegal	22nd April, ,,
Tralee	28th April, ,,
Galway	12th May, ,,
Limerick	14th May, ,,
Waterford	1st July, ,,
Wexford	21st August, ,,

It is worthy of remark that Dublin, Cork, and Belfast were affected about four months before Waterford and Wexford. Now a steamer plies twice a week between Dublin and Cork, and Dublin and Belfast, *while there is no direct communication by steam* between Dublin and Waterford, or Dublin and Wexford; and consequently it appears probable, from the dates, that Cork and Belfast were infected from Dublin, while Waterford and Wexford escaped for many months, not being exposed to infection from this source. At all events, the fact that Waterford and Wexford should have remained so long without the disease is very remarkable, and, if not sufficiently accounted for by their more indirect and less frequent intercourse with Dublin, it may perhaps be explained by their trade with England consisting chiefly of the export of agricultural produce, rather than the interchange of passengers.

LECTURE XXVIII.

CONTAGIOUS CHARACTER OF CHOLERA.—TREATMENT.

We have hitherto followed the route of cholera in the old world; we have now to trace it in the new.

"The disease commenced about the 8th of June, 1832, in Quebec, in boarding-houses and taverns in the *Cul de Sac*, a low, uncleanly, and ill-ventilated part of the city, crowded with emigrants of the lowest description, with sailors and other persons of irregular habits."*

Thus we find that cholera appeared in America first at Quebec, just at the season when the spring stream of emigration from England reaches that city. The following account proves that cholera might be thus transmitted:—

"The following letter from the surgeon of the British barque "Brutus," to the President of the Board of Health of Liverpool,† conveys the melancholy intelligence of the cholera having broke out among the passengers *eight days after* leaving the river Mersey, and which induced the captain to put back. It appears from a statement subjoined to the letter, that between the 27th of May, the period when the first person was attacked, and the 13th of June, the day on which the vessel arrived at Liverpool, 117 cases had occurred, eighty-one died, and twenty had recovered.

"'With the deepest feelings of regret, I have the painful duty to perform of transmitting to you one of the most melancholy and distressing accounts of cholera, which occurred on board the British barque "Brutus," bound for Quebec, from Liverpool, with three hundred and thirty passengers. The first case presented itself on the 25th of May (being the eighth day after we left the river) in a strong, healthy man, thirty-five years of age; the symptoms were all well marked, the spasms particularly severe;

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* See the official Report of the Board of Health, Quebec *Cholera Gazette*, p. 72.

† *Cholera Gazette*.

under the usual means of treatment he recovered. The next case was an old woman of sixty, who died in ten hours after the commencement of the attack. The disease continued gradually to increase (notwithstanding every means having been employed to arrest its progress) until the night of Saturday, the 2nd of June, when we were a good deal tossed about by a heavy sea, and dark hazy weather; it spread to such an alarming extent that on Sunday, most of the ship's crew being attacked, and having lost some of them the week before, we were obliged to bear up again for Liverpool. It is impossible to describe the scene of misery on the 3rd, 4th, and 5th—people dying in every direction—the greater number of them destitute of the common articles of bed-covering. On the 6th the weather became more favourable, the disease less severe, and the number of new cases diminished, which has since been on the decline.

"'W. W. Thompson.'"

On the 10th of June, 1832, it appeared at Montreal, and here, as at Quebec, it immediately assumed the character of a most destructive pestilence.

The following interesting account* of the route of cholera during the first stages of its progress in North America, is from the pen of S. Jackson, M.D., Secretary to the Consulting Medical Board of Philadelphia. Dr. Jackson is a non-contagionist, as will abundantly appear from his narrative, upon some of the leading facts of which I may hereafter take occasion to make a few observations. It is worthy of remark that the medical men of America have far outstripped their European colleagues in medical statistics. The weekly, monthly, and annual accounts of diseases, deaths, &c., in each of their great cities have been long published systematically and regularly, and that with a degree of accuracy to which we are strangers. Some of the results of this praiseworthy habit appear in Dr. Jackson's account.

"From the numbers of emigrants who, about this period, had landed at Quebec, and arrived at Montreal from England and Ireland, a first impression was created that they had been the means of transmitting the epidemic across the Atlantic. A more close investigation into the facts connected with the commencement of the disease in these cities served to destroy this supposition. It could not be traced to importation. The emigrants and lower classes of the Canadians were attacked simultaneously in both cities. Numbers of the emigrants were in circumstances eminently predisposing them to suffer attacks of the disease, and they and the lower Canadians were precisely the description of persons most obnoxious to the ravages of epidemic cholera, and such as have been universally observed to be its first victims.

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* *Cholera Gazette.*

"The lines of communication between the cities of Quebec and Montreal, and the cities of the United States, are by the Richelieu river, Lake Champlain, and the northern canal leading to Troy and Albany; or by the St. Lawrence to Lake Ontario, to Buffalo, and by the Erie Canal leading to Rochester and Albany. It was confidently expected that the disease would penetrate into the United States from Canada by these routes. Along the first, many cases of the disease did certainly occur in the persons of emigrants, but they terminated without its communication to others. On the contrary, the epidemic manifested a decided predilection for the shores of the St. Lawrence, successively attacking the towns and villages along its banks, then following the borders of Lake Ontario until it entered Lake Erie.

"While attention was directed to the northern and western boundary, supposed to be threatened by the invasion of the disease, it suddenly and most unexpectedly appeared in the city of New York.

"The first case occurred, it is said, on the 24th of June, when a man, a native citizen, residing at the corner of Gold and Frankfort Streets, was attacked by the disease. Four cases soon succeeded, the location of which was in Cherry Street. The subjects were Irish emigrants, who had arrived in Quebec in the autumn of 1831, and had resided in Albany until the month of May, when they removed to New York.

"On the 27th of June the disease manifested itself in Belvue Almshouse, distant about three miles from the city. The patient was an aged woman who had not left the house for three years, who had held no communication with the city, and no admission into the ward she occupied had taken place for a month. Several cases immediately ensued in this and the other wards of the house. The epidemic reached its maximum in this establishment on the 11th July, and terminated on the 4th August.

"In the city of New York the climax of the epidemic arrived on 11th of July, from which period it continued very steadily to decline.

"The time that elapsed from the outbreaking of the epidemic at Quebec and its appearance at New York is a period of sixteen days, or nineteen at Belvue Almshouse. The distance between the two cities in a direct line is four hundred and fifty miles.

"It is to be remarked that all the intermediate cities on the seaboard of the province of New Brunswick and Nova Scotia, of the states of Maine, Massachusetts, and Rhode Island, remained entirely exempt from the epidemic; and even to the present period, except in Providence, Newport, and Boston, no cases have as yet appeared.

"In this city the epidemic was much more tardy in its progress than it had been in the Canadas or in New York. The first decided case of cholera occurred on Thursday, July 5th. A man of the name of Musgrove, residing in the cellar of a house in Filbert Street, near Schuylkill, Fifth Street, was attacked with symptoms of malignant cholera on that day. This man had but lately been discharged from the New Jersey prison; he had been affected with diarrhœa for two or three weeks previous to the cholera symptoms. The disease proved fatal on Sunday the 8th. The next case was a black man residing in St. John Street, Northern Liberties, above Callow Hill. He had been employed working on board a ship from England lying at Pratt's Wharf. He was seized with symptoms of malignant cholera the night of Tuesday, July 9th, and died on Friday. This man was perfectly sober in habits; no premonitory symptoms existed.

"No other cases presented themselves until Sunday, July 14th, when two females, occupying a room in a dwelling in Coate's Street, were the victims of the pestilence in its most aggravated shape. Both these females were exemplary in their habits of life, but appeared to be infirm in health. The husband of one of these unfortunates had arrived on Saturday, July 7th, from New York, exceedingly alarmed respecting the cholera. He was taken sick the next day, and died on the succeeding Friday. On

Saturday the widow felt unwell, and without advice took sixteen grains of calomel in the evening. She was soon afterwards seized with vomiting and purging, and in the course of the night she sunk into collapse. She died Sunday night. The mother of the deceased husband on Sunday morning complained of feeling unwell, but without any definite symptoms. Having been up with her daughter-in-law during the night, her uncomfortable feeling was attributed to fatigue. She was then going about the house, and had been out on an errand. She was requested to lie down as a matter of precaution, and a small dose of opium administered to her. This was at eight o'clock in the morning. Dr. Schott, who was in attendance an hour afterwards, went up to her chamber to inquire into her state. He found her lying on the floor; copious dejections of rice-water looking fluid had occurred, and she was in complete collapse: death ensued in the evening. These were the only cases to which the slightest suspicion of communication by contagion could attach; but on the same day a Frenchwoman, temperate in habits, about fifty years of age, living in Kensington, beyond the close built part of the town, at the head of West Street, was also a victim of the disease. This woman had not been from her dwelling for three weeks; her house is isolated, being surrounded by kitchen-gardens for the supply of the market. She had been affected with diarrhœa since Friday, for which she had dieted, but had taken no medicine. The case proved fatal next day.

"From this time not more than three or four cases occurred, all scattered in different quarters, particularly Kensington, Northern Liberties, and Southwark, until the 27th and 28th July, when the epidemic fairly set in, and cases continued daily to be developed. The disease attained its height in this city on the 5th, 6th, and 7th of August, since which time it has gradually declined, and appears now to be extinct.

"Taking the 27th or 28th of July as the proper commencement of the epidemic in Philadelphia, there will be a period of twenty-four or twenty-five days intervening between its first appearance in New York and this city. The distance in a direct line is about ninety miles.

"A comparative view of the population, number of cases, and deaths, in the cities which have been brought under observation, presents the epidemic in an interesting point, and exhibits in a clear manner the character it assumed in this city.

Date of Report and Place.	Population.	Cases.	Deaths.	Ratio of Cases to Population.	Ratio of Deaths to Cases.	Ratio of Deaths to Population.
Sept. 30—Quebec	32,000*	5,783	3,292†	1 in 5¼	1 in 1½	1 in 10½
„ 1—Montreal ...	28,000‡	4,385	1,853	1 in 6½	1 in 2½	1 in 15⅙
Aug. 22—New York...	140,000§	5,547	2,782‖	1 in 25⅓	1 in 2	1 in 15½
Sept. 13—Philadelphia	160,000¶	2,314	935	1 in 70	1 in 2½	1 in 173·$\frac{22}{183}$

"The results of this table show conclusively, that the causes productive of cholera were less numerous in the city of Philadelphia than in Quebec, Montreal, or New York, or were so modified as to possess a much less degree of activity. The causes of this result, so favourable to Philadelphia, important in the hygienic history of cholera, and consoling to humanity, as placing this formidable affection to so great an extent under control, it is interesting to investigate.

"The following are the circumstances which, existing more particularly in Philadelphia, may be regarded as influential in ameliorating the violence of the epidemic cause, circumscribing its activity, and diminishing its fatality.

"1. The plan on which the city is built, arranged in hollow squares, separated by wide and paved streets, prevents excessive crowding of the inhabitants, procures free ventilation, and gives facility to the means of cleanliness. It is to be regretted that any deviation has been permitted in the original design of Penn, whose sagacity and foresight has been so amply demonstrated in the circumstances of the late epidemic.

"2. The abundant supply of wholesome water, placed at the command of the whole community, affords a healthful beverage, and gives the means of the most complete cleanliness, by washing the dirty gutters of the streets, close alleys, and lanes.

"3. The well arranged measures of sanitary police, devised and actually carried into effect by the councils of the city, and the boards of commissioners of the district, and the sanitary committees appointed by them, and by the Board of Health. The measures consisted in a thorough investigation into all existing nuisances, and in their immediate abatement; in a complete system of cleanliness of the city steadily pursued; in the early establishment of numerous local hospitals, provided with ample medical attendance, nurses, and every means applicable to the treatment of the disease; and in spreading before the public early information, derived from the consulting medical committees, of the methods—hygienic, dietetic, and medicinal—best adapted for guarding against the attack of the disease, or to arrest the symptoms at its onset.

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* "Permanent population, 27,000; transient population, 5,000.—Total, 32,000.

† "Protestant grounds, 1,244; Catholic cathedral, and cholera grounds, to 25th September, 1,574; at St. Roch, 470.—Total, 3,292.

‡ "Permanent population, 25,000; transient population, 3,000.—Total, 28,000.

§ "Estimated as remaining by Mr. D. Leslie.—*Journal of Commerce*, August 8th.

‖ "Report of the Inspector.

¶ "Population within the bill of mortality."

"4. A very considerable influence may be attributed to the annunciation made by the mission sent to Canada, immediately on its return, and before the epidemic had commenced its career in this city, of the different periods of the disease, and especially of the existence, in almost every instance, of premonitory signs and a preliminary stage, with a description of the symptoms indicating its existence. This information was communicated to the public by the sanitary committee through the daily journals of the city, by handbills liberally distributed, and by placards on the corners of the streets. The Board of Health adopted the same measures, and pursued the same course. In this manner the whole community, before the beginning of the epidemic, was instructed in the most important points in the general knowledge and management of this affection—its commencing period, the premonitory symptoms, its general curability in that state, the necessity of immediate attention and medical advice, and the methods of relief. These facts had been overlooked, and this attention to the instruction of the public was entirely neglected in Quebec and Montreal, and in New York. From being taken unprepared by the epidemic earlier than was anticipated, they were not communicated to the public until the measure had been adopted in this city, and when the epidemic there had already attained its maximum of intensity.

"5. The moral resolution, calmness, and a perfect freedom from alarm and panic generally manifested by our citizens, and inspired by a thorough confidence in the efficacy of the preventive means enforced, in the advantages for salubrity of the city, and in its medical resources, contributed in no small degree to diminish the number of cases and the intensity of the attacks. No stores were closed on account of the epidemic, and not more citizens left the city than usually abandon it every summer. A stranger entering our streets, from the busy throng and cheerful aspect of all he met, would never have suspected the existence of an unusual and a desolating scourge.

"6. The treatment of the disease generally pursued in the city, in the preliminary stage, had most probably no small share in preventing the development of the disease in innumerable instances. In the lighter forms, it was limited chiefly to diet, rest, tranquillizing doses of anodynes, or mild diffusibles, with occasionally the mildest laxatives or gentle cathartics, conjoined with sinapisms or other rubefacients. The drastic and perturbating cathartics were seldom if at all prescribed, and the stimulant practice but rarely resorted to.

"The foregoing circumstances appear to us as those principally instrumental in producing the favourable results attending the epidemic in this city. As such they acquire a high degree of interest, and afford most instructive lessons as regards the measures of municipal and civil regulation connected with sanitary police.

"In its general features and character, the disease differed in no respect from the many descriptions that have been made since it first attracted attention in Asia, and subsequently in its progress through Europe. It will be unnecessary to make the repetition here; it is, however, important that the fact should be signalized, that during the prevalence of the epidemic very few persons in the city were entirely exempt from some derangement or disorder of the digestive functions. It is not probably exaggeration to assert that two-thirds of the population were affected in this manner, which is to be attributed entirely to the epidemic influences. It should also be stated that, in the majority of cases which assumed the decided character of malignant cholera, preliminary symptoms had existed, varying in duration from a few hours to several days. In those rarer instances which were not preceded by any premonitory signs, the subjects were the aged, the intemperate individuals, who had committed some great imprudence in diet, or whose constitution has been enfeebled, and such cases were generally, if not universally, fatal.

"The chief mortality of the disease existed in the public institutions. It was much lighter in private practice. The following table exhibits the cases of deaths, as reported in private practice and the public institutions. The reports, however, do not exhibit the results of private practice in as favourable a light as they really were. A considerable number of physicians in the most respectable practice reported only the cases that proved fatal, or exceedingly severe. They did not return to the Board of Health the lighter cases, which yielded to the operation of remedial measures. The mortality of private practice in the reports appears, in consequence, to have been far greater than it really was.

" *Table of Cases and Deaths, with Ratio as occurring in private practice, and the public institutions.*

	" Cases.		Deaths.		Ratio of Deaths to Cases.
" Private Practice	1,175	...	270	...	1 to $4\frac{3}{16}$
" Hospitals..................	874	...	342	...	1 to $2\frac{5}{9}$
" Almshouses...............	174	...	92	...	1 to $1\frac{41}{46}$
" Arch Street Prison	86	...	46	...	1 to $1\frac{20}{23}$

" Had the returns of cases in private practice been complete the proportion of cases would have been much greater; it would have ranged probably as 1 to 70 or 80, or even more.

"In the hospital practice, the first cases introduced were nearly all fatal. This circumstance is to be accounted for from the universal observation, wherever cholera has prevailed epidemically, that the worst constitutions were the first to suffer attacks. In the commencement of the epidemic, persons first attacked, unaware of their danger, and the nature of the affection, neglect application for aid, and resist the offer of hospital assistance until reduced to a hopeless condition. Besides, misled by the authority of the English and Scotch writers, extensive means had been prepared for warming the patients by heated air, steam, and other means. Experience in a short time proved the pernicious effects of this system. The patients succumbed most rapidly under the exhaustion induced by the profuse watery exhalation from the skin caused by this treatment."

Why the cholera, *if an imported disease*, should have broken out nearly simultaneously in Quebec and Montreal, is very easily accounted for, since both are the receptacles of British and other foreign emigrants; on the same principle, we must explain its appearance so soon after at New York, where, no doubt, it arrived by a separate importation from Europe—a circumstance which will prevent us from feeling the same surprise with Dr. Jackson, that between Quebec and New York *all the intermediate cities on the sea-board escaped,* at least for a few months. This is analogous to the exemption of Waterford and Wexford, during several months that cholera raged in Dublin and Cork. I gave Dr. Jackson's Report at much length, because it is intended to be conclusive against the theory of contagion; while it, in my opinion, contains strong internal evidence of a contrary tendency.

In the United States cholera spread far and near, as might be expected from the wonderfully rapid and frequent intercourse that takes place all over the Union; but, except in the condensed population of the chief seaports, its ravages were not great. It is curious to observe how little Philadelphia suffered in comparison with Montreal, Quebec, or New York; no doubt because its population is less condensed, and live in families more separated from each other. In making this observation, I do not mean to undervalue the power of predisposing causes, such as poverty, bad diet, intemperance, &c., which prevail more in the latter cities than in Philadelphia. Still, comparing America with those European and Asiatic countries which suffered most, the only constant difference we can discover is, that the separation of families is much more complete in the United States than in any other country except England; and to this difference, consequently, we are justified in referring for an explanation of the remarkable fact, that England and the United States fared better than other countries, notwithstanding their acknowledged superiority above all in the facilities of internal communication. A wish to be brief forces me to conclude the subject of the cholera in North America with the following list of places, and the dates of its arrival in each:—

Albany	3rd July,	1832.
Troy	16th July,	,,
New Brunswick	July,	,,
Rochester	July,	,,
Baltimore	August,	,,
Washington	August,	,,
Boston	August,[*]	,,

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[*] I am not certain of the dates of its first appearance, where the day of commencement is not mentioned; but in all the above places the cholera prevailed during the above months.

reached Lisbon sooner: in America how quickly it extended from one seaport to another!

It is a remarkable circumstance, and one which ought to have great weight in the discussion respecting the contagiousness of cholera, that *cholera has in no recorded instance appeared in any place sooner than the ordinary modes of communication might have brought it from some infected station.* Again, it can easily be proved that *the rate at which cholera travels varies with the rapidity of that communication.* A few weeks were sufficient to transport it from the ports of Britain more than three thousand miles across the Atlantic to Canada, while it took six months to creep along the interrupted line of communication between Oporto and Lisbon.

From the preceding observations it will appear, 1st, that cholera has had no fixed rate of progress; 2nd, that it has spread in every direction, sometimes northwards, sometimes southwards, and other times east and west, its route being determined not by the points of the compass, but by the great lines of internal and international communication.

Cholera never got to any of the West Indian Islands, nor to British (formerly Dutch) Guiana, Demerara, nor any of the embouchures of the great South American rivers, Amazon, Orinoco, or La Plata, though the soil and climate, with the immense tracts of inundated and swampy lands, would there seem most favourable to its development.

In September, 1835 (*Lancet*, vol. for 1834—5, p. 782), "the cholera had nearly ceased its ravages in the south of France, and took a south and easterly direction along the countries bordering the Mediterranean Sea. It penetrated into Piedmont in spite of the strictest precautions, and prevailed with more or less intensity at Nice, Coni, Livorno, Genoa, Florence." From this extract we do not learn the dates of its arrival at the above places, but they were probably according to their respective distances from France. The kingdom of Naples was not infected until a still later period; at Naples, probably, September, 1836. It attained the maximum at Naples on the 22nd November, 1836; Algiers, 14th October, 1837; Bona, September, 1837.

To trace it accurately, its secondary routes and dates of reappearance should be made out; it would then be found to have returned often on its steps.

November the second principal Burman town, Tavoy. Tavoy is a place of considerable size, and is situated about one hundred and fifty miles south of Moulmein, on the bank of a broad shallow stream, loaded with debris from the neighbouring mountains. Cholera raged here with great fury for three or four months, and then gradually disappeared. Soon after entering Tavoy it was heard of in the villages round the city, and, travelling south, it showed itself shortly afterwards (some time in January, 1843) in Mergui, the third principal town in the provinces, situated on a small island formed by two branches of the Tenasserim river, opening into the Bay of Bengal, about one hundred and fifty miles to the south of Tavoy."

It prevailed throughout the Indian provinces at intervals during the next two years, and early in 1845 it raged with great violence along the banks of the Indus, and also in Affghanistan. Thence it spread into Persia, Tartary, Hindostan, and the pachalic of Bagdad. In May, 1846, it broke out with frightful severity at Teheran, carrying off as many as 300 a-day for several weeks, and reducing the population of that town by at least 20,000 souls.

From this town it proceeded in two directions, one south-west in the line of Ispahan, Shiraz, and Bagdad; and the other north-west to Tabreez. In October some cases occurred at Saliam and Lankeram, frontier Trans-Caucasian towns of Russia. In the south it spread along the Tigris; and in December it raged with great violence at Mecca, "*being supposed to have been conveyed thither by the pilgrims from Bagdad.*" Early in the year 1847, it appeared to the west of the Caucasus, in the Russian army fighting against the Circassians. "By the middle of May it was at Tiflis, and also at Astrachan at the mouth of the Volga; and where it reached its greatest intensity about the end of July. The towns of Kars and Kutais also, lying westward of Erivan and Tiflis, with many of the surrounding villages, were attacked about the same time. In August it broke out at Batoum on the eastern shore of the Black Sea, and soon afterwards at Erzeroum and Trebizonde, to the southward; reaching the last-named city about the 9th of September. Shortly before this time, it had appeared at Taganrog, Kertsch, Mariopol, and other towns on the Sea of Azoff, and near the mouth of the Don; subsequently spreading in a northerly direction towards the more inland provinces of Charcow, Kiev, &c. Again were all the most stringent preventative measures found to be utterly ineffectual in arresting, or even in slackening, the progress of the disease. By the Russian official reports in the middle of September, we learned that it was gradually spreading more and more into the heart of the empire, by two distinct lines; one more northerly and along the course of the Volga towards Saratoff, Tamboff, Kasan, Toula, and Moscow; and the other from the north shores of the Black Sea along the lines of the Don and Dnieper, and their numerous branches. The general direction of the epidemic has been north-westward; and it has been remarked that the route followed in the present year has been very nearly that along which the 'disease-producing something' travelled in 1831. On the last day of September it appeared at Moscow, and about the same time at Odessa and Perekop, on the north-western shores of the Black Sea, having previously ceased, or nearly so, at Taganrog, Mariopol, and other parts to the eastward. In the middle of October, we are told by official returns that, without counting Georgia, the Caucasus, and the country of the Cossacks of the Black Sea, the disease existed with greater or less severity in sixteen different governments of the Russian empire. At the same time it was announced that it had again broken out in some parts of the north of Persia, as Tabreez, Khoi, &c., and also at Bagdad.

"In the second week of November the *St. Petersburgh Gazette* stated that 'the most western points the cholera has yet reached are the town of Alexandrof in the government of Kherson, and the district of Olgapol in Podolia,' which is not above thirty miles from the Austrian frontier. To the northward it had been travelling from Moscow to Novgorod in the direction of the capital, and also in a course nearly due west to Dwinaberg, at a very little distance from Riga, and within forty miles of the Prussian territory. A letter from Vienna of the 20th ult. announces that some cases had occurred in the circle of Tarnapol in Gallicia."

This latter extract I have taken from a pamphlet published by Dr. Gavin Milroy of London, which contains an excellent concise history of the cholera epidemic.

The next account which we have of this epidemic I read from the Russian *Cholera Gazette* of January 29th, 1848. Dr. Thielmann, writing in it, says: "During the month of December the severe cold so completely arrested the progress of Asiatic cholera, that there was reason to believe it would disappear entirely. It has altogether ceased in the provinces around the Caspian; and, with the exception of Moscow, Mohilew, and Witepsk, it is no longer met with in any of the great cities or towns of the empire. Even in these and in smaller places the disease has assumed so mild a character, that it appears to be on the point of extinction.

"Letters from Constantinople, of the 1st January, announce the gradual disappearance of cholera in that city. The epidemic was then chiefly confined to the Arsenal; and out of 210 attacked only 58 died. Accounts from Bagdad, of the 7th of December, state that the cholera had almost entirely disappeared from Kerkoula and Suleymania. Letters from Mossol, dated the 12th of December, mention that the cholera had ceased in that city, after having killed 300 persons; and intelligence from Aleppo, of the 18th, states that it has appeared at Beregik, on the banks of the Euphrates, and was causing from ten to fifteen deaths daily."

Then, as in the previous epidemic, it was, however, only smouldering, to break out ere long with great fury, and probably pursue the same route it did in 1831—32; for, according to an official account received in the beginning of June of this year (1848), there had been no fewer than three hundred thousand persons attacked, of whom one hundred thousand perished, and the proportion of persons attacked was to the population nearly as great as on the former visitation. Its progress was in all respects similar to that of 1832, when it abated on the occurrence of the frost, and reappeared in the spring, and proceeded onwards. It had declared itself at Nijni-Novgorod and at Moscow. At Moscow there were twelve cases and five deaths between the 8th of April and 12th of May; and at Novgorod, twenty-two cases and twelve deaths between the 17th and 24th of April.

Let me now, before concluding, call your attention to some points in the treatment of this contagious pestilence. When cholera existed in Dublin in the spring of 1832, the modes of treatment principally relied on were, bleeding in violent spasmodic cases, emetics of ipecacuanha and mustard, the application of heat externally and internally, stimulants, but, above all, calomel, not in small but in large and frequently repeated doses, either alone or combined with opium. I need not tell you that the mercurial treatment came to us sanctioned by high authority; it was a remedy to which the experience of Indian practitioners had given a high character, but in our hands, I must say, it proved of very little value. Be this as it may, I must say that I had reason to be dissatisfied with this mode of treatment; I had tried it myself, and had seen it tried in every way which ingenuity or experience could suggest, but I had seen it fail almost in every instance.

About the middle of summer the epidemic began to spread fearfully among those who had hitherto been exempt from its attacks; many persons in respectable life were seized, and my private practice afforded numerous opportunities of becoming practically acquainted with the disease. In several cases to which I was called in, the malady had not advanced to the stage of collapse; the symptoms of cholera, properly so called, had merely commenced; the intensity of the disease was still far away, and a fair chance was afforded for the operation of therapeutic agents. In most instances I tried calomel and all the ordinary remedies with profitless results; my treatment proved too often ineffectual; and some persons whose lives I highly valued perished in spite of all my efforts, leaving me grieved for their loss, and mortified by my own want of success. I found that I could no longer place any confidence in calomel, and determined in my own mind to give up a remedy which had so signally failed; it was, however, a question of deep anxiety to me, what I should select instead, or to what article in the Materia Medica I should have recourse, where so many had proved utterly valueless.

About this time I happened to be called on to attend a case of obstinate diarrhœa with the late Dr. Hunt. The case was an extremely harassing one, and had resisted all the ordinary remedies. I advised the use of acetate of lead and opium in full doses; this was given, and I had the satisfaction of finding that the diarrhœa soon yielded. Before this period I had received a letter from that able practitioner and excellent man, Dr. Bardsley of Manchester, directing my attention to the use of acetate of lead in large doses, in that form of diarrhœa which occurs towards the termination of long fevers, that is to say, the diarrhœa which precedes and accompanies inflammation of the glands of the small intestines. I had subsequently, at Sir Patrick Dun's Hospital, several opportunities of witnessing the truth of Dr. Bardsley's remarks. I saw that in many cases during the course of fever, where the patient was low and prostrated, symptoms of intestinal congestion came on followed by diarrhœa, which many persons thought would end in ulceration of the glands of Peyer; and I found that in such cases the acetate of lead was the only remedy that could be relied on. I observed, too, that, contrary to the prevailing opinion on the subject, it could be given in large doses with perfect safety. You are aware that Dr. Bardsley has shown that it may be given to children in very considerable doses without any bad effects, and that in adults he has pushed this remedy to the extent of twenty or thirty grains in the day, without any unfavourable consequences.

With these impressions I came to the resolution of trying the acetate of lead in the next case of cholera which offered a chance of deriving benefit from any kind of treatment. It is known that there are some cases in which the disease at once assumes so frightful a malignity, that the patient is lost from the very moment of his seizure. This hopeless and intractable malignity is not peculiar to cholera; it is seen in fever, scarlatina, croup, measles, and hydrocephalus; in fact, there are certain forms of all diseases in which the best directed efforts of medical skill not only fail in curing the disease, but even in retarding its progress. But there are cases of cholera where the patient is not struck down at once, where the disease is not developed at once in all its awful intensity, and where time, brief though the space may be, is allowed for the play of therapeutic agencies. It is in such cases the acetate of lead may be given with some prospect of success, and it is by such cases alone, and not by those which are necessarily fatal *ab initio*, that its value is to be tested.

Before we proceed further, I may observe that the principle on which the calomel treatment was employed in cholera arose from almost constantly observing that there was a total deficiency of bile in the stools. Soon after the supervention of an attack, the alvine discharges were observed to be white, and without the slightest tinge of bile; and on this very remarkable symptom practitioners dwelt almost exclusively, thinking that the patient's only chance lay in restoring the secretion of the liver. Now it is obvious that the absence of bile in the stools is no more a cause of the disease than is the deficiency of urea in the kidneys, or of serum in the blood. Viewing the disease in this light, it would be just as reasonable to give a diuretic to restore the secretion of the kidneys as to give calomel to produce a flow of bile. The liver ceases to secrete, not only in consequence of the injury done to its vitality by the proximate cause of cholera, whatever that may be, but also from a mechanical cause, namely, from a diminution in its supply of blood.

It may appear strange that when the same given number of vessels go to the liver and come from it in all times, that the quantity of blood circulating in it should be greater at one time than another. I have not time at present to enter fully into this subject; but it is a fact admitting of sufficient proof that the quantity of blood circulating in any organ is very much modified by the state of its capillaries. The quantity of blood also which goes to a gland varies according to the peculiar state of that gland, being greater during its period of active secretion than when it is at rest. But in a case of cholera, where the capillary vessels of the intestinal canal from the stomach to the rectum are actively engaged in taking up the serum from the whole mass of blood, and pouring it into the cavity of the digestive tube, there is an enormous drainage from the system, and there must be consequently a deficiency of blood somewhere. Now it would appear that a quantity of blood, sufficient for the purposes of secretion, is abstracted not only from the biliary, but also from the urinary system; and hence, it appears just as reasonable to give diuretics to restore the urinary secretion as to give calomel to excite the secretion of the liver. It would be, *a priori*, as original a mode of treatment, and be equally as successful. I have therefore no hesitation in saying, that the calomel treatment has no claim to merit on the ground of theory, and, as far as I have observed of it in this country, it seems to be of no practical value in the treatment of cholera.

With regard to the quantity of acetate of lead which may be given in this disease, and the mode of administering it, a few words are necessary. I have already stated that when I first tried it, I prescribed it in large doses, fortified by the authority of Dr. Bardsley, and by my own experience of its utility in many cases of diarrhœa. It appears that before I recommended the acetate of lead, it had been used at the Cholera Hospital in

Grangegorman Lane. Of this I was not aware, until a book was subsequently published by Dr. Cranfield, which I afterwards reviewed in the sixth volume of the *Dublin Journal of Medical Science*, and I feel that on that occasion I did fair and impartial justice to its merits. I certainly did not know that the acetate of lead had been given at the Grangegorman Hospital; for in the very able report of cholera, as observed at that institution, published by one of its officers, Mr. M'Coy, the treatment relied upon appears to have been the mercurial, and not a word was said of acetate of lead. It had been used there by one physician, but it was given in small doses, insufficient to produce decided effects, and no stress had been laid on its value as a remedy in cholera by the practitioners attached to the hospital. Be this as it may, acetate of lead was not known to the medical men of Dublin and to the practising apothecaries before I recommended it. It had been frequently employed in the form of injection by them; but no one had given it in large doses by the mouth, or introduced it to the notice of the profession. I believe I can fairly claim the merit, such as it is, of being the first to give it in large and effectual doses.

The mode in which I administered it was this: a scruple of the acetate of lead, combined with a grain of opium, was divided into twelve pills, and of these one was given every half hour, until the rice water discharges from the stomach and rectum began to diminish. In all cases where medicine promised any chance of relief, this remedy was attended with the very best effects. It gradually checked the serous discharges from the bowels, and stopped the vomiting. I need not say of what importance this is: as long as these exhausting discharges continue, as long as the serum of the entire body continues to be drained off by the intestinal exhalents, what hope can we entertain? What benefit can be expected from calomel and stimulants, when every function of the digestive mucous membrane seems to be totally extinguished, except that of exhalation, and while profuse discharges, occurring every five or ten minutes, are reducing the patient to a state of alarming prostration? Knowing the inevitable fatality of all cases where these discharges went on unchecked, I was happy at having discovered a remedy which seemed to possess more power in arresting them than any yet devised, and this impression was confirmed by the results of subsequent experience.

The march of influenza has not as yet been mapped out; from the accounts which have reached us in 1837, it seems to have travelled at the same time in very different directions, arriving at Cape Town in January, during mid-summer, and in London in the same month, during mid-winter; while it is reported to have reached New Holland, and to have raged among our antipodes, two months earlier, and in 1847 it pursued the same variable course.

It is obvious that influenza does not depend upon mere variations of temperature, for we have had many seasons as changeable as the present, without the occurrence of any such epidemic. Besides, influenza is known to be a disease which travels through the most different climates, preserving its peculiar character and identity in all. It is not to be supposed that the same temperature, or the same barometrical and hygrometrical conditions of the atmosphere, prevail here as in Spain, France, Germany, or Sweden; yet in all these countries the influenza has exhibited a uniformity of character, and an identity of type, proving beyond all doubt that it is one and the same disease. That influenza is not produced by a low temperature is proved by the occurrence of the disease in the month of June, in that of 1762; and in the months of May and June, in that of 1782; as well as by its appearance at the Cape of Good Hope in the middle of summer, as I have already noticed. In 1837 influenza increased rapidly in this city, while the weather was remarkable for its serenity and agreeable mildness. In London many were led, by a limited view of the subject, to consider its origin as connected with the breaking up of the frost and the peculiar state of atmosphere attending a general thaw. Influenza is not influenced in its progress by situation or locality; it does not creep along the shores, or follow the course of large rivers, or select low marshy districts in preference to drier and more elevated soils.

From what has been said, it is obvious that influenza does not depend upon vicissitudes of temperature, peculiarities of situation, or supposed moist or dry states of the atmosphere; neither does it arise from the prevalence of certain winds, for meteorological observation furnishes many instánces of the prevalence of such winds without any influenza; and, on the other hand, it frequently travels against the wind. The same views are also advocated by the late Dr. Holland; at page 184 of his "Medical Notes and would indicate the average return of influenza once every ten years.

In making calculations of this kind, medical writers should always take care not to confound influenza, a disease which spreads rapidly over the whole globe, regardless of season and climate, with those local catarrhal affections that occur in all temperate climates almost annually. One thing, at least, is certain with respect to this disease, that it does not arise from exposure to cold, or, as it is termed, from catching cold. This I have repeatedly observed. Persons who took the best care of themselves, who always went warmly clothed, and were never exposed to the inclemency of the weather, took the disease just as readily as the half-clad labourer, who had to undergo daily exposure to all the vicissitudes of our changeful climate. But it should be observed that although the attack of influenza in any individual was not necessarily dependent on exposure to cold, yet in many instances it was evident that catching cold determined the immediate access of influenza, or increased its violence when present.

I have also observed that it seldom attacked persons labouring under acute diseases until the period of convalescence arrived, when their immunity ceased, and they became just as liable to its invasion as others. Thus, patients labouring under typhus escaped as long as the fever continued; but frequently, on the very day the crisis occurred, and symptoms of returning convalescence appeared, they were seized with influenza. This is a very unfortunate circumstance. Just as a patient had struggled through a fever of seventeen, nineteen, or twenty-one days, he was attacked with a new and dangerous malady, which again placed him in a situation of imminent danger.

You must have observed that influenza does not appear in every individual with the same violence, or exhibit in all symptoms identical in their intensity or duration. As in most other epidemics which affect society at large, the different constitutions and ages of the individuals, and the different states in which the morbid influence finds them, modify greatly the nature of the attack; so that although a vast number are affected, they suffer in very different degrees, and the complaint exhibits every variety of shade, from simple coryza, or catarrh, requiring no treatment, to catarrhal fever of the worst and most unmanageable sank afterwards under various diseases, of which influenza had laid the foundation. In Paris the same epidemic influenza caused likewise a great mortality; for it appears from a statement in the *Révue Médicale*, that the average daily mortality, during the first fifteen days of February, amounted to one hundred and ten, which is more than double the usual average. This only refers to persons dying in their own houses, and does not include the deaths in the hospitals. Eighteen thousand die in private houses annually in Paris—*i.e.*, on an average, about fifty daily. The rate varies from twenty to seventeen a day, according to the season; but during the first fifteen days of February it rose from fifty-eight to one hundred and fifty-two in the day.

I have obtained a similar return to that of 1837, for the months of December, 1847, and January, 1848, being the months in which the late epidemic of influenza prevailed in Dublin; it is as follows:—

Prospect Cemetery, Glasnevin.

1846—November	571	1847—November	697
December	867	December	1,141
1847—January	756	1848—January	912
February	700	February	786
Total for four months	2,894		3,536

Increase during Influenza, 642.

This return does not include those who died in the fever sheds and North Union Workhouse, amounting to 215 in the month of December, 1848, alone, and many of which were doubtless cases of influenza; but, by omitting them, we avoid the increased mortality caused by the epidemic of fever which then raged, and thus obtain a nearer approximation to the truth.

This return was most kindly furnished to me by Mathias J. O'Kelly, Esq., the present Secretary of the Cemetery Company. To it I am enabled to add a similar return from Mount Jerome Cemetery, Harold's Cross: very few poor persons are interred in this cemetery.

In November, 1846......	55	In November, 1847......	66
December, 1846	113	December, 1847......	124
January, 1847......	90	January, 1848......	104
February, 1847......	74	February, 1848......	72
Total for four months	332		366

Increase during Influenza, 34.

Influenza in 1837 was very fatal where it attacked persons who had been subject to chronic bronchitis, or who had happened to labour under any form of asthmatic affection; for this, I confess, I was not quite prepared. And when first called to attend asthmatic persons labouring under influenza, I expected that, from being accustomed to periodic attacks of dyspnœa and cough, they would be better qualified to bear the disease, and would continue to exhibit that tenacity of life for which asthmatic persons are so remarkable. The old also suffered considerably; but some very old persons had extremely severe attacks of influenza, and yet escaped. I attended, along with Mr. Maurice Collis, the venerable Judge Day, the cotemporary of Goldsmith, who, at the age of ninety-three, had sufficient strength of constitution to shake off a most violent seizure. Two gentlemen, who had fought at the battle of Bunker's Hill, also survived the disease in a severe form; but generally speaking, it was very fatal among the aged. Influenza was also very fatal among persons who laboured under disease of the heart; and in this instance age made no difference as to result, for the young and old were equally liable to danger. I have also seen it fatal in cases of deformity of the chest, from curvature of the spine and other causes. The mortality was also very great among persons in advanced life who laboured under tussis senilis: in a word, all persons labouring under pulmonary irritation, or weakness, were exposed to very considerable danger. Subsequent experience has proved also, that where influenza left behind an obstinate and irritating cough, and where the constitution had a scrofulous taint, the disease was very apt to pass into tubercular phthisis. Among all the families I know, but two escaped the influenza altogether: one consisted of eleven children, besides the parents and servants, and resided in Pill Lane, in the very centre of the city; the other family consisted of five females, advanced in life, and who lived in one of the fashionable streets.

Very nearly similar remarks apply to the influenza of 1847, but the depression of the powers of life was, I think, much more marked, while the feverish symptoms were less than in 1837. Consonant with this observation, I remarked that death occurred with symptoms of *paralysis* of the lungs in all the cases which I saw that terminated fatally: this appeared to be the manner in which the peculiarly depressing influence of the epidemic was manifested.

Allow me to digress here for a moment, for the purpose of making one observation, which a review of several cases of influenza, attended with severe pulmonary symptoms, suggests to me. It is a common error in pathology to confound effects with causes, and where the cause of a disease is not and probably cannot be known to fix on some peculiar and leading symptom, and to attribute to it the origin of all the rest. But it is quite illogical to say that one symptom is the cause of another, or that because it has the precedence, it should also have the initiative. I alluded to this error in a former lecture, when speaking on the pathology of scarlatina. It has been over and over again asserted, that the dropsy of scarlatina arises from the previous inflammatory affection of the skin, or subcutaneous tissues; and the same thing has been asserted with regard to the desquamation of the cuticle. But I have brought forward facts and arguments to prove that this opinion is not founded in truth, and that dropsy, as well as desquamation of the cuticle, may take place where there has been no eruption whatever, and not the least trace of cutaneous or subcutaneous inflammation.

Now, when a person, after exposure to cold, gets pneumonia or bronchitis, followed by anasarca, it is quite a common thing to hear it said that the anasarca had its origin in the pulmonary affection, and that the effusion of serum depended on obstructed transmission of blood through the lung. The same mode of explanation has been applied to disease of the heart as the cause of dropsy. This explanation, however, appears to me inadequate and unsatisfactory. Many cases of influenza were accompanied by extreme congestion of the lungs, and consequently imperfect aëration of the blood; and yet I have not in a single instance noticed the occurrence of dropsy as an immediate or remote consequence. Were dropsy dependent on the state of the lung to which I have alluded, it would have shown itself in some cases at least; and yet I have seen individuals attacked with influenza labouring under orthopnœa and severe pulmonary symptoms for weeks, without observing, in any instance, the slightest anasarca or œdema. In one case, indeed, that I saw, an old gentleman at Rathmines, the feet and legs were much swollen; but this I attributed to his having remained so many days and nights in his chair, unable to lie down. This has strongly impressed upon my mind the conviction, when dropsy comes on after disease of the lung, that the one is not always the consequence of the other, but that both often result from the same cause, and owe their origin to the same morbid impression on the system. This error has been further confirmed by the results of treatment, practitioners having found that measures adapted to remove congestion of the lung tended also to remove the dropsy ; forgetting here, that where two symptoms closely allied arise together from the same cause, you will be most likely to remove both by those means which are effectual in removing either. The principle which I have here briefly alluded to will apply to many other combinations of disease ; it is one of general application and, in my mind, of no ordinary importance.

The epidemic of 1837 differed in many points from that which prevailed about three years previously. The influenza of 1833—34 was by no means so generally fatal as then, and in 1847. It was characterized, like both, by considerable irritation of the tracheal and bronchial mucous membrane, but not by the severe bronchitis and pnuemonia which have been witnessed in later cases of the epidemic. The former raged in Dublin chiefly during the months of March and April. It came on very suddenly with rapid pulse, hot skin, great prostration, languor, and excessive sweating; there were cough, coryza, and not unfrequently vomiting at the commencement. One of the most prominent symptoms, however, was headache, which was excessively severe. There was also, *cæteris paribus*, more debility, and the patients did not bear bleeding so well as they did in 1837. But the most material point in which they differed was the comparative mortality. The disease in 1834 carried off some very suddenly with cerebral symptoms, and proved fatal to others from oppression of the chest and dyspnœa. Few, however, died who survived for a week after they had been attacked, and the disease rarely left behind it a cough at all approaching in violence and obstinacy to that which in the later epidemics harassed convalescents. On the whole, the fever accompanying the influenza of 1834 was more acute, and set in with more marked depression of the nervous system, and the disease was much less liable to become chronic.

It would conduce greatly to the advantage of medical science, if a brief and accurate history was left to posterity, of the character, symptoms, pathological phenomena, and treatment of every epidemic. Such a record would prove a guide and beacon to the practitioners of future ages—would enable them to draw important comparisons between the existing and the past—and thus arrive at a more fixed and available knowledge of the nature and habits of epidemic complaints.

There are, I have no doubt, many curious forms of epidemic disease which pass through society either wholly unnoticed, or confounded with others to which they have some slight affinity. I think I have seen particular forms of scarlatina, measles, small-pox, and fever, which have not been accurately noted, although they prevailed as epidemics. If every form of epidemic was noted, and the order of its succession marked, it would remain to be ascertained by posterity whether there may not be what may be termed cycles of epidemics, and whether disease, after having manifested itself in determinate forms, following each other in determinate succession, may not commence again after the lapse of a certain number of years, and pursue the same course. This is not impossible, if we suppose that epidemics are connected with telluric or electrical influences, which are known to observe a periodic course. Were this ascertained, a sort of observatory of epidemics could be easily established in the various civilized states.

In treating of the nature of influenza, it will be proper to consider in the first place the general constitutional symptoms which attend it, and afterwards glance at those which are chiefly of a local description. In some cases of influenza there is little or no fever, as in the last epidemic; neither does the presence of fever seem essential to the more severe or even fatal cases, although, generally speaking, fever occupied a very prominent position among the group of symptoms by which the disease was characterized in 1837. I have seen cases in which there was nothing like regular fever from beginning to end, and yet which terminated fatally.

I remember treating two patients who had been labouring under orthopnœa for ten days, and yet in these patients the skin was cool, the pulse in general soft and very little above the normal standard, and the tongue, though furred, quite moist; but so great was the distress of respiration, that they were obliged to remain sitting up in bed night and day, panting for breath. This, however, was the exception with respect to severe cases in 1837, the majority being attended with very considerable fever. In the slight cases the fever was scarcely perceived, or altogether absent; as was the case with myself and some of my friends. We had coryza, hoarseness, cough, and some degree of pulmonary irritation without any fever. At first I thought that fever was an essential part of the disease; but the cases to which I have alluded, and others of a similar kind, have convinced me that this is not the fact—a conviction fully ratified by the late influenza.

Where the fever appeared it came on with the usual symptoms of pyrexia—namely, sense of chilliness, particularly about the small of the back, without decided rigors, flying pains in the limbs and joints, and headache, generally referred to the situation of the frontal sinus. There were from the commencement great restlessness, jactitation, and more or less insomnia. Sickness of the stomach, loss of appetite, and tendency to diarrhœa were also common symptoms. The skin was in general hot, and without any tendency to moisture, although in some cases there were occasional perspirations. These, however, were seldom general or regular, and lasted only for a few hours. The pulse was accelerated and tolerably full, occasionally even hard and wiry. These symptoms were very subject to slight exacerbations and remissions, and seldom continued the same for more than twelve hours together. Where the disease existed for any length of time in a violent form, the tongue usually became furred and loaded, the patient lost all relish for food, and in many cases complained of harassing thirst.

In severe cases the most prominent symptoms were cough, wheezing, restlessness, dyspnœa, and loss of sleep. The appetite was in general more or less impaired; but I have seen some severe cases in which it did not fail remarkably for several days; the restlessness and jactitation attended many cases throughout. You are not, however, to suppose that this always depended on the presence of pain or fever. The headache was not in all severe or distressing; and I have already stated that the fever was not so general or so violent as one would suppose. The loss of sleep depended upon derangement in the tone of the nervous system, independent of fever; for I have observed it in numerous patients, in whom scarcely any febrile excitement was observable;

but when complicated with fever, both react upon and aggravate each other. The skin, where fever was present, was hot: this heat was interrupted by occasional perspirations, which, however, did not give much relief, or tend to diminish the amount of increased temperature. Sometimes the skin was hot, and at the same time bedewed with perspiration during the whole course of the disease; but this was rather unusual.

The pulse in influenza is seldom the same throughout; one time you will find it quick and rather hard; in six hours afterwards it will be quick and soft; in six or eight hours more it will appear as if about to fall to the normal standard; and next day you will find it quick and jerking again. These changes are accompanied by corresponding alterations in the temperature and humidity of the skin. But what is most remarkable with regard to the pulse is, that it sometimes becomes full and rather strong and wiry, towards the termination of the disease; and this you will observe in patients who have been suffering for days, or even weeks.

I attended, with the late Mr. Colles, a gentleman in Castle Street, aged 60, of a full habit, and subject to attacks of dyspnœa and cough during winter. This gentleman was attacked with influenza, ushered in and accompanied by severe fever; and it was observed that as the disease advanced his pulse became fuller and stronger, so that it was thought advisable to bleed him. He was bled with apparent relief, and the blood was extensively buffed and cupped. This phenomenon I have observed in every case attended with fever, and indeed in some where no appreciable fever existed. Thus, in a gentleman in Dame Street, who had no fever, and who merely laboured under teazing cough, distress of respiration, and oppression of the chest, the blood on being drawn exhibited very distinct buffing and cupping. The same thing happened in the case of a gentleman in Dominick Street, whom I ordered to be blooded under exactly the same circumstances.

The gentleman in Castle Street, whom I attended with Mr. Colles, exhibited a very curious state of pulse. In him, as in many others, the pulse was extremely variable as to its strength, being at one time hard and firm, and at another soft and weak. If you were to visit him in the morning, from the feel of the pulse you would be inclined to give him stimulants: if you saw of the influenza. We have rarely any authority for it in the state of the pulse, which neither in strength nor frequency bears relation to these inflammatory symptoms; while the difficult or painful respiration which often suggests the remedy furnishes evidence against its fitness, by becoming frequently more laborious than before—the effect of larger accumulation in the bronchial cells, and of diminished power."

I shall now mention the particulars of a very remarkable case which came recently under my notice. I was called to visit a lady somewhat advanced in life, but of a good constitution, and labouring under the ordinary form of influenza, with considerable dyspnœa and cough. In the course of eight or nine days her symptoms began to decline; she got up, and seemed convalescent. As the cough and pulmonary irritation still prevailed to a certain extent, it was thought advisable not to allow her to eat meat, but she obtained leave to take some fresh haddock. After dinner her cough becoming more troublesome than before, she had frequent recourse to a stale and rancid cough bottle containing squill and ipecacuanha. During the evening and night she felt her dinner like an undigested load, and her stomach turned. She vomited, and was purged and griped incessantly, until I saw her next day. On the third day the medicines I had ordered moderated the purging, but the nausea and occasional vomiting continued. On the fourth day the purging had entirely ceased, but the sickness of stomach persisted. I sought to appease this by the ordinary means, which failing, I examined her on the following day, and discovered a strangulated hernia. At this time the pulse had scarcely risen above the natural standard. Mr. Cusack operated that night with his usual skill, and all the symptoms depending on incarcerated hernia ceased. But they had scarcely disappeared when the pulmonary symptoms and the copious secretion from the bronchial tubes recurred, and she did not survive this relapse of the influenza more than a few days.

This is an instructive example of an insidious combination of circumstances very likely to mislead a practitioner; for as the vomiting was for a day or two accompanied by a looseness of the bowels, the suspicion of hernia would not strike the attention. It is plain that in this case indigestion produced an increased and morbid activity in the motions of the alimentary canal, which led to the incarceration of the portion of gut. Up to a certain moment the symptoms depended merely on one cause; after that period strangulation took place—an occurrence which could not be easily diagnosed, as vomiting, one of the most striking symptoms, had previously existed.

When diarrhœa occurs, it is generally at the commencement of the disease; and it is remarkable that this state is frequently exchanged rather suddenly for one of an opposite character. Thus, when you have succeeded in checking the diarrhœa with chalk mixture and opium, a state of costiveness frequently ensues, requiring the daily use of purgatives and enemata. I have now witnessed several cases in which the moderate use of opiates and astringents brought on constipation, requiring the use of strong purgatives, and enemata thrown up with Read's syringe.

In influenza, as in many other febrile affections, the lungs become considerably engaged; the disease first attacks the nose and throat, then the larynx and trachea, and finally the ultimate ramifications of the bronchi. There are several other affections which commence in a similar way—as ordinary catarrh, bronchitis, and measles. In influenza most persons have the nose and throat affected in the beginning; the inflammation creeps gradually along the lining membrane of the air passages, until it involves the greater part or the whole of the bronchial mucous membrane. The progress of the inflammation is extremely rapid, and in the course of twenty-four or even twelve hours, the lungs become engaged.

There is, however, much difference as to the extent to which this inflammation proceeds. In many cases it is limited to the nose and throat; the patients complain of coryza, hoarseness, and slight cough. In others the trachea also is more or less affected, and the cough is more troublesome; but, generally speaking, the latter as well as the former cases are unattended with fever. The patients eat and drink as usual, go about their ordinary business, and sleep tolerably well at night. This appears to be the general course of the disease when the inflammation is limited to the nose, throat, and upper part of the air passages; when it spreads farther and attacks the first ramifications of the bronchi, there is some dyspnœa and tightness of chest, the cough is much more troublesome, and the appetite and digestion are somewhat impaired; but persons in this state, although resting badly and eating but little, will continue to go about—constantly, however, complaining that they are very ill. When the smaller divisions and ultimate ramifications of the bronchi are engaged, there are soreness of chest, remarkable dyspnœa, and constant harassing cough; the headache is also aggravated, the patient loses all inclination for food, sleeps badly at night, and is confined to the bed or house.

First, then, you have the mucous membrane of the eyes, nose, and throat affected, then the larnyx and trachea, then the larger bronchi, and finally the smaller and more minute ramifications. When the latter state has continued for some time, more or less serious engorgement of the lung takes place, and this adds to the dyspnœa and cough. On applying the stethoscope over the lungs, you will hear at various parts a moist crepitus, indicating the existence of serous infiltration. The smaller bronchial tubes and air vesicles are congested and filled with mucus; the blood cannot pass freely through the lung, and consequently must be imperfectly aërated; the secreting and absorbing functions of the lung are deranged; and hence arises a state in which the pulmonary capillaries become congested, and permit the more fluid part of the blood to exude into the parenchyma of the lung, giving rise to what is termed serous infiltration.

Something similar to this occurs also in bronchitis, particularly in fever, but we very seldom have hepatization resulting from such causes. In hepatization the capillaries pour out, not serum, but lymph, which glues together the cells of the pulmonary tissue, and forms a dense solid mass. Hence in influenza or bronchitis you seldom have true pneumonic inflammation. You will have extensive and dangerous engorgement, but when you examine the lung after death, you do not find any real solidification, and you can restore the lung almost to its original permeability and buoyancy by squeezing out the infiltrated fluid. Yet I must admit that this is not always the case, and that in influenza, as well as in bronchitis, you may have true pneumonia superadded to the original affection of the lining membrane. This occurred in the case of a lady whom I attended in Capel Street, and who was attacked with influenza shortly before delivery. On the day of her accouchement pneumonia was superadded to the bronchial inflammation, and she died with extensive hepatization of the right lung. This also occurred in the case of a man of middle age, residing in Suffolk Street, who had been labouring for some days under excessive engorgement of the lung. I have also observed the same occurrence in a gentleman whom I attended with the late Mr. Colles in Exchequer Street, and in another case which I saw in Whitefriar Street.

One of the most singular features in the history of influenza is the extraordinary degree of dyspnœa witnessed in most cases where the lung is extensively engaged, but particularly where the patients had been previously subject to pulmonary affections; and even in many cases where the bronchial mucous membrane is but slightly engaged, the amount of dyspnœa is remarkably great. Indeed it might be said with much truth that the dyspnœa was by no means proportioned to the extent of pulmonary inflammation. There was a case in the hospital of a woman labouring under influenza, whose chest sounded clear on percussion, and in whom every part of the lung was permeable, who presented nothing more than a few sonorous rales in the course of the larger bronchial tubes, and yet she was suffering from considerable dyspnœa, and the respirations amounted to forty-six in a minute. We cannot, therefore, attribute the difficulty of breathing to mere bronchitic lesion, for it was not in proportion to this lesion. Another patient admitted into Sir P. Dun's Hospital exhibited a similar train of symptoms. He was a negro sailor, a native of New Brunswick, and was seized with the epidemic a few days after his ship arrived in Dublin; he was a man of Herculean form and finely developed chest, and in the prime of life. His suffering from dyspnœa was intense; his chest heaved, he tossed about in bed in a constant state of agitation and restlessness, and yet the respiratory murmur was everywhere distinctly audible through the lung, and no rale could be heard, except here and there a few bronchitic wheezings. He also laboured under insomnia, and, though he had but little fever, debility was extreme. Indeed his pulse was so weak from the commencement, that I could not venture to treat him antiphlogistically; and I accordingly ordered extensive vesication over the chest, with the use of wine, stimulants, and narcotics. This man subsequently recovered—an event which could scarcely have occurred under the plan of treatment adopted, had his dyspnœa depended on mere bronchitis.

It should be also borne in mind that, in many bad cases of influenza, the dyspnœa is intermittent, or at least undergoes remarkable exacerbations and remissions at certain hours of the day and night. It would appear that the respiratory derangement depends on the same general cause which produces the whole train of symptoms, and that it might exist even where there was no bronchial inflammation at all. It is true that, where the bronchitis is present, it adds to the distress of respiration, but the dyspnœa appears to be chiefly attributable to some impression made on the vital activity of the lung. That the lungs are endowed with an inherent vitality necessary to the aëration of the blood has been long acknowledged by the Germans, who have described a dyspnœa from paralysis of the lungs; and this opinion is now generally adopted in Great Britain since the results of the experiments on the eighth pair of nerves have been duly appreciated. We have abundant illustrations of this truth in asthma, in which the greatest dyspnœa is often present, without any appreciable lesion of the lung. And it would be a fortunate circumstance for the patients in influenza if this were not the case; for we could then treat the affection of the lung as ordinary bronchitis, and should expect to find it amenable to the ordinary remedies.

You are aware that the mortality in cases of ordinary bronchitis is extremely small, if we except very young children and persons advanced in life. In adults, when met by prompt and appropriate treatment, it is generally a very manageable disease, and seldom proves fatal, unless combined with other unfavourable conditions. This, however, is not the case in influenza, nor is the pulmonary affection so easily treated, or the dyspnœa so readily controlled. I saw, some time ago, a fine young woman, servant to a gentleman in Fitzwilliam Street, for whom everything had been done which the best and most skilful practice could devise; but her condition when I saw her was desperate, and she died the following day; yet her chest sounded well on percussion, and we could hear nothing over the whole lung, except a few sonorous and sibilous rales, and the respiratory murmur seemed everywhere nearly as loud as natural. Of course, such a lesion of the nervous influence could not last long without necessarily inducing pulmonary congestion—an inevitable consequence of imperfect aëration of the blood. When the eighth pair of nerves is divided, the animal is slowly suffocated; and, on dissection, the lungs are found engorged, and the bronchial mucous membrane congested and inflamed. May not the affection of these parts in influenza be sometimes induced by lesions of nervous power in the lungs?

To the late Dr. George Green, Professor of the practice of Physic to the College of Physicians, I am indebted for the following results of his very numerous post-mortem examinations in this disease, and I feel great pleasure in being able to give them, as such examinations, at least in this country, are very rare:—

"The cases which proved fatal at the House of Industry during the late epidemic influenza (1837), occurred principally among the aged inmates of both sexes. I had an opportunity of examining several of these cases, and the following were the principal post-mortem appearances observed.

"The bronchial mucous membrane was found in every case more or less congested and inflamed. The colour varied considerably, being in some of a dull red, and in others of a much darker hue. The inflammation, in most cases, was found to occupy both the trachea and the bronchial tubes of both lungs; in other instances, it was confined to one lung alone. A sanguinolent frothy mucus occupied the area of the tubes, and increased in quantity as they were traced to their minuter divisions. The parenchymatous tissue of the lung was invariably discoloured, being generally of a dark or violet colour; its specific gravity was increased, and it did not crepitate, or at least very feebly, when pressed between the fingers. The surface of its section was not rough to the touch, and when pressed in the hand, a quantity of the mucus described above was driven out. In some cases, the postero-inferior portions of one or both lungs were very dark coloured, and the finger could be passed easily through the substance. When the surface thus torn was examined, it did not appear to be granular; it resembled more a portion of gangrenous lung, except that there was an absence of fœtor. This last appearance was found principally in very aged persons. It was rare to find any traces of the second and third stages of ordinary pneumonia in these patients; but in the young and robust, who were received into the Hardwicke Fever Hospital from the neighbouring streets, these degenerations of the structure of the lung were observed, together with the same inflammation of the bronchial mucous membrane.

"In most of the aged patients, the blood was found dark coloured and fluid in both cavities of the heart, and in every vessel where it was examined. The cases in which fibrinous concretions in the cavities of the heart were found were very few, and these invariably in the young or middle-aged. In the former class of patients, also, the lung occasionally appeared to be œdematous; and in one or two cases, a considerable effusion of serum had taken place into the pleural cavities. The signs of recent pleuritis were very rare, but old adhesions, as might be expected in such subjects, were very commonly found between the pulmonary and costal pleuræ. In one case of a lunatic, who survived the immediate attack of influenza, tubercles appeared to have been rapidly developed in both lungs. In another lunatic, two tubercular cavities were found, in addition to the state of the lung and air-tubes already adverted to.

"With respect to the nature and duration of the symptoms of those cases which came under my own management, I have little to say in addition to what is already so familiarly known. The physical signs afforded by percussion and auscultation were almost universally as follows:—Dulness, more or less decidedly marked, in the postero-inferior portions of the lungs; sonorous or some form of bronchial rale throughout the chest, or, what was more common, a mixed sonorous and crepitating rale, or in the latter stages, a muco-crepitating rale. The sputa were seldom rust-coloured or tenacious, but rather resembled those of bronchitis. In many cases, the want of power to excrete them appeared to be the immediate cause of death; but in others, the morbid cause, whatever it might be, appeared to have affected the entire respiratory and circulating systems, producing great congestion of the venous system, and a state not unlike asphyxia. The latter cases were almost all among the aged inmates of the House of Industry.

"The appearances of the other viscera were not such as could in any way account for the result, so often speedily fatal; so that, so far as one could hazard a conjecture, the morbid cause appeared to have made its primary impression on the respiratory mucous surface, thereby interfering with the proper aëration of the blood, and inducing the changes in that fluid and in the structure of the lungs above detailed."

Such were the appearances observed by Dr. Greene in

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numerous dissections of persons who died of influenza. They may be relied on as perfectly accurate, for no one was better acquainted with pathological phenomena than Dr. Greene, and consequently no one better able to furnish valuable evidence with respect to the appreciable changes produced by influenza in the pulmonary and other tissues.

I have already advanced the opinion, that we should not hastily assume that influenza consists essentially in the morbid changes which dissection reveals; we should examine every side of the question, and consider whether it is not possible that the alterations in the pulmonary tissue may not be, to some extent at least, the consequences of the disease. Let us consider for a moment the method we pursue in reasoning about the progress and causes of the symptoms in ordinary bronchitis. Here a patient is seized with a pectoral affection, attended with cough, dyspnœa, and more or less fever. We find certain rales, and the expectoration is altered in quality and quantity. Further, observing a number of such cases, we remark that the danger is proportioned to the degree of dyspnœa, and the dyspnœa to the extent and nature of the rales, together with the quantity and quality of the expectoration. To these the general constitutional affection, and the probable results of the disease, have certain definite relations, a knowledge of which is soon obtained by experience.

But these rales, and this state of the respiration and expectoration, we have reason to believe, arise from the presence of bronchial inflammation; and to this we refer all the symptoms observed. On this supposition, too, we proceed in our treatment, and the result most commonly justifies its correctness; and we have additional evidence of its truth furnished by post-mortem examinations. Now, in such instances, the chain of inductive evidence is complete, and we feel a conviction that our practice is founded on correct notions of the nature of the disease. But how different is the case when we assume that influenza is caused by bronchial inflammation! In influenza the dyspnœa is not always proportioned to the bronchitic affection—nay, in some cases we have seen that difficulty of breathing was most urgent in cases where the air entered into all parts of the lung with facility, and where few and unimportant rales existed. Again, although the presence of a copious viscid secretion in the bronchial tubes was sure to aggravate dyspnœa, yet it often occurred in patients whose air-passages were very little, or not at all obstructed in this way. The effects, too, of remedies, antiphlogistic, expectorant, and derivative, were very different from what they would have been had the disease depended on a mere bronchitis. I have already stated my conviction, that the poison which produced influenza acted on the nervous system in general, and on the pulmonary nerves in particular, in such a way as to produce symptoms of bronchial irritation and dyspnœa, to which bronchial congestion and inflammation were often superadded.

In this view of the subject I am not singular, for I find that it has been advocated by Dr. Peyton Blakiston, in a short treatise on influenza as it occurred at Birmingham. He states that his researches have led him to the conclusion "that influenza is an affection of the nervous system, with its concomitant derangements in the organs of digestion, circulation, &c., commonly known under the name of nervous fever, accompanied throughout its whole course by irritation of the pulmonary mucous membrane, which not unfrequently amounts to congestion, and even to inflammation."

This distinction between influenza and feverish cold with bronchitis is, in a practical point of view, of great importance, and should never be lost sight of in the treatment of influenza, for it prevents us from placing our sole confidence in remedies adapted to mere bronchitic inflammation. Thus Dr. Blakiston asserts, and most physicians will agree with him in this point at least, that it was often necessary to have recourse to diffusible stimulants at the commencement, and to administer tonic medicines in an early stage of the disease.

In some cases, even when dyspnœa exists, the cough is hard and dry, and the expectoration scanty; in others the expectoration is copious, so as to cause constant efforts to cough it up; and, indeed, it is melancholy to look at the distress which patients suffer in this respect. You will hear the wheezing of the phlegm in the throat and air-passages before you enter the room, and you will see the patient exhausted by successive paroxysms of cough, and ineffectual attempts to expectorate. In other cases, where the vitality of the lung is less injured, and the general tone of the system less deranged, the sputa, although copious, are expectorated with considerable facility.

The sputa bear considerable analogy to those observed in ordinary bronchitis. They consist at first of a greyish mucus; as the disease proceeds, they exhibit a globular appearance, or assume a puriform character, and do not coalesce. In other cases they are extremely viscid and ropy, like solutions of gum or isinglass. A remarkable fact with respect to the sputa in influenza is that they are very seldom mixed with air-bubbles. On mentioning this to some persons attending my class, I was shown some sputa discharged by a patient labouring under influenza, in which there were some air-bubbles; this, however, is extremely rare. In a lecture which I delivered some time ago, I took occasion to allude to the secretions of the bronchial mucous membrane, and stated my conviction that this subject had not received as yet the attention which its acknowledged importance demands. There is one point in particular, of which no adequate explanation has as yet been given—namely, why it is that in some cases of pulmonary inflammation the sputa are filled with air-bubbles, while in other instances there is no appearance of air-bubbles from the beginning to the end of the disease?

The presence of air-bubbles in the sputa has been explained by supposing that air becomes incorporated with the mucus, while it is driven up and down in the bronchial tubes during the acts of respiration and coughing; just as if you shake a solution of soap or any other viscid fluid in a half empty bottle, it becomes impregnated with air-bubbles. There may be some truth in this, but I think it does not sufficiently explain the presence and intimate incorporation of air with the sputa in certain affections of the lung; and it appears to me that we can scarcely understand this, unless we suppose that the air and mucus are secreted together. You are aware that air is secreted by the bronchial mucous membrane, and that in some cases this secretion is morbidly increased, in others morbidly diminished. Now, it is not very unreasonable to suppose that the mucous membrane may secrete air and mucus together in abnormal quantity; and that this, rather than any mechanical agitation, may be the cause of the intimate combination of air with the expectorated fluids.

I need scarcely make any observations on the cough in influenza. It is in general very troublesome, particularly at night. Many persons are not much annoyed by it during the day, but at night it becomes very harassing, and prevents them from sleeping.

When severe, it continues both night and day; and even when persons have recovered from the fever and dyspnœa, and are able to go about, the cough will continue extremely troublesome: this I have observed in the majority of cases. In this state medicines prove of very little service, and one of the best remedies is to change to a mild country air. Cases of cough, in which I have tried every remedy without success, and which had resisted every form of treatment in the city, yielded in a few days to the salubrious influence of change of air.

In influenza the urine is generally much loaded with lithates, and contains a large quantity of uro-erethrine or purpurine. It is red when voided, deposits a good deal of sediment, and tinges the vessel in which it lies with a pink film. It bears some resemblance to the urine which accompanies arthritic and gouty affections. In very bad cases, this state of the urine continues up to the period of death. You recollect what I stated with regard to the condition of the blood; it is generally buffed, even where there is scarcely any febrile excitement in the system, and thus affords a very fallacious indication. The same observation holds good with respect to the state of urine and temperature of the skin. I may observe here that the heat of the skin is very variable; it is sometimes very high, sometimes natural: in fact, like the pulse, it falls and rises in a very remarkable manner, at certain times in the day.

I have already spoken of the affection of the mucous membrane of the bowels. I may observe that in some cases of influenza the morbid influence is translated to the brain, and symptoms of delirium or coma supervene. Thus, in two instances that have been communicated to me, the patients fell into a state resembling coma during the course of the disease. In three cases witnessed by the late Mr. Swift, the attack of influenza terminated in a train of symptoms bearing a close analogy to delirium tremens, and requiring the use of blisters to the head and nape of the neck, full doses of opium, purgative enemata, wine and the occasional use of mercurials. The patients complained of great headache, noise in the ears, intolerance of light, and more or less sleeplessness from the commencement, along with the usual pulmonary symptoms. After five or six days they became excessively nervous, lost all sleep, had continued subsultus and tremors, and talked very incoherently, particularly at night.

During the prevalence of the cerebral symptoms, the pulmonary affection partially or wholly disappeared, but returned again in some degree after the subsidence of the delirium. All these cases terminated favourably.

I believe I have already remarked that many persons who have laboured under very severe pulmonary symptoms will struggle through the disease; and I may mention here, that I have seen persons recover who have suffered from continued orthopnœa for three weeks. Still the mortality, particularly among the aged, is very great; and I fear that we shall shortly have but few octogenarians to tell the occurrences of the last century. Indeed, the mortality has not been confined exclusively to the aged, for many persons in the vigour of life have sunk under the attack. There have been several deaths among the soldiers in our garrisons, notwithstanding the excellent state of health which our troops enjoy, and the skilful and judicious treatment of our present army surgeons.

In many individuals influenza has laid the foundation of other and very serious diseases, and this I especially witnessed in the epidemic of 1847. In some, the diseases so produced could be traced to the depressing effect on the nervous system. Thus, Dr. Mulock informs me that in three cases which he attended, relapse from exposure to cold terminated in insanity, which in one of the cases ended fatally.

It now remains for me to say a few words concerning treatment. First, as to bleeding. A great deal was expected from general bleeding, because the disease was sudden and violent in its onset, and accompanied by symptoms which seemed to require active measures—such as an inflammatory state of the bronchial mucous membrane, accompanied by quick pulse, hot skin, and high-coloured urine. This led persons to expect much benefit from venesection. The results, however, of its employment are, generally speaking, unsatisfactory. Where venesection was employed promptly and in the beginning of the disease, and where it seemed to be strongly indicated by the buffed and cupped state of the blood, even in such cases it has failed to afford anything like material or permanent benefit, or to produce a decided amelioration of the existing symptoms. The general impression among practitioners in Dublin seems to be, that bleeding is doubtful in its effects, if not altogether improper. I am much inclined to think that bleeding, unless employed within the first twelve or twenty-four hours, will be likely to do as much, or more, harm than good. Bleeding on the second or third day, except to relieve congestion of the lungs, seems inadmissible. The same observation holds good with reference to other diseases. Thus, in scarlatina, if you happen to be called when the rigor commences, and while the disease is beginning to form, you will sometimes accomplish much good by bleeding your patient; but after eighteen or twenty-four hours, when the disease is fully formed, venesection will not do. On this point I can speak from experience. In scarlatina, the difference of a few hours renders venesection inapplicable, and even injurious. It is the same thing with respect to influenza; general bleeding is useful only in the commencement; and where the symptoms seem to demand it, it should be employed at least within the first twenty-four hours.

Where I have been fortunate enough to find the disease just commencing, I bleed to the amount of twelve or fourteen ounces, order the patient to remain in bed and take some aperient, followed by the use of nitre. In this way, by timely bleeding, aperients, sudorifics, and confinement to bed, the attack generally passes over in two or three days. I could mention many instances of the success of this plan of treatment. In one family I treated all the individuals attacked in this way, and I have done the same thing in many cases of persons somewhat advanced in life. In the case of an old gentleman who was very severely attacked, I succeeded by these means in checking the disease at once. My experience, therefore, is, that bleeding is of service in the very commencement of the disease; but as it seldom happens that a physician is called in at this period, I would qualify my statement by saying that, as a general measure, bleeding in influenza is seldom admissible.

When you are called on to attend cases, you will most generally find that the patients have been ill for two or three days or more; and then the only mode of abstracting blood, which you can have recourse to with safety, is by leeching. About eight or ten leeches applied over the hollow of the neck, just above the sternum, and allowed to bleed pretty freely, will prove very serviceable; and if you apply them in the evening, you will often secure to your patient a good night's rest. This plan of leeching n the hollow of the neck, in cases of tracheo-bronchial inflammation, is an excellent one: the leeches are applied at a spot which lies close to the trachea, and particularly to that point to which the irritation accompanying bronchitic affections is chiefly referred.

By the aid of leeching, the use of aperients if necessary, and confinement to bed, with sudorifics, you will frequently succeed in removing the fever and bronchial inflammation. You will derive much benefit, particularly in the early stage of influenza, from tartar emetic and nitre; but I must say that neither leeching nor tartar emetic and nitre proves as valuable and as efficacious in influenza as in ordinary bronchitis. Some of my friends, who use tartar emetic as a nauseant in the commencement of the disease, inform me that they have derived benefit from its use; and others have told me that they have used tartar emetic and opium, in the commencement and during the course of the disease, with advantage. I have not employed the first of these, but I have the latter, and with favourable results. You may therefore, after using antiphlogistics for a day or two, proceed to the use of opiates in combination with tartar emetic or nitre. In some cases the camphorated tincture of opium will answer very well; in others, you will find the acetate or muriate of morphia better. A mixture, composed of six ounces of almond emulsion, a drachm of nitre, and half a drachm or more of the liquor muriatis morphiæ, will be found very useful. The muriate of morphia, which possesses many of the valuable properties of opium without its defects, will serve to tranquillize the system and produce sleep—two most important points in a disease like influenza, connected with increased nervous irritability.

A gentleman on whom I place much reliance tells me that he has treated many bad cases successfully with camphor mixture, tincture of opium, and tartar emetic. I need not mention the various remedies which have been recommended in this disease—as Mindererus's spirit, Hoffman's anodyne, ipecacuanha alone or combined with extract of conium and blue pill, and many other remedies belonging to the class of diaphoretics or expectorants. They are all more or less serviceable, but they have all the common defect of producing less relief than they usually do in cases where the pulmonary affection is simple and idiopathic. Towards the end of the disease you find it necessary to give stimulating expectorants and light tonics—as infusion of polygala senega, infusion of columba, &c.

One word about blisters before I conclude. They are useful in some cases, but in many of the severe ones they do little or no good, and only add to the patient's sufferings. They do not relieve the pulmonary symptoms, and particularly the dyspnœa, in the manner you would be prepared to expect. I do not know a more remarkable circumstance in the present disease, than the failure of blisters; and in many cases I do not employ them at all. Fomenting the trachea and chest with very hot water appears to be much more serviceable. This has proved extremely valuable in many cases of this as well as other affections of the air-passages.

LECTURE XXX.

THE CONNEXION BETWEEN DISEASES OF DIFFERENT ORGANS.

In order to acquire a correct and available knowledge of human pathology, and to extend the range and confirm the accuracy of diagnosis, it is of the utmost importance to observe attentively the connexion between the diseases of certain organs or systems of the body. You are aware that some organs, when labouring under disease, are apt, after the disease has continued some time, to implicate other organs, giving rise to various deranged conditions, which are developed, sometimes simultaneously, but in general consecutively, and in sequence. I have already pointed out several diseased actions thus associated together, each forming a link in the morbid chain. Now it is of the greatest importance to study each link, and ascertain the nature of its connexion, so as to have a distinct conception of the whole.

Let me first direct your attention to a train of morbid phenomena sometimes observed co-existing with arthritic inflammation. A person labouring under inflammation of the joints gets an attack of hepatitis, accompanied by jaundice, and this is followed by urticaria. I have observed this sequence of disease in eight or nine cases. The first was in a gentleman residing in Lower Mount Street, whom I attended with Dr. Cheyne. This gentleman, in consequence of exposure to cold, was attacked with arthritic inflammation and fever. After he had been about ten days ill, he became suddenly jaundiced, and in a day or two afterwards a copious eruption of urticaria appeared over his body and limbs. Exactly the same train of phenomena, and in a similar order of succession, was observed in a man treated in the Meath Hospital in 1832. A short time before this, I had been attending a medical friend in Baggot Street who had been affected in the same way; and I mentioned to the class, as soon as I perceived the man was jaundiced, that he would most probably get urticaria. I made a similar prediction in a case which occurred recently in our wards, and it was verified by the event. Now this is not a mere fortuitous occurrence; the various symptoms must be connected in the relation of cause and effect. It is interesting to bear this in mind, and it is besides of considerable importance to the practising physician; it enables him to predict the appearance and form of disease, and inspires his patient with confidence in his opinions and judgment.

Since my attention has been drawn to the connexion between these three diseases, I have seen and heard of several other instances in which they appeared thus associated together. A circumstance so remarkable deserves to be studied with more than ordinary interest. Let us, therefore, consider what facts are supplied by physiology and pathology capable of throwing some light upon this hitherto unobserved and uncultivated subject. In the first place, nothing has been longer recognized by physicians, as an established fact, than the intimate sympathy which exists, both in health and disease, between the digestive organs and the skin. Now, acute hepatitis always produces more or less derangement of the stomach and alimentary canal, and we may therefore consider its connexion with urticaria in the same way that we are in the habit of viewing the cases, so frequently observed, in which certain sorts of fish have produced serious symptoms of indigestion followed by nettle rash. The association between these two diseases is rendered more remarkable by the fact, that when fish taken as food exerts a poisonous effect on the system, it frequently produces not merely violent stomach and bowel complaint, but also inflammation of the joints and rheumatic pains. If I can establish this, you will allow that the connexion between arthritis, disease of the digestive organs, and urticaria can no longer be considered as fortuitous, and depending on the accidental concurrence of causes having no determinate relation; but must be looked on as owing to and arising from the operation of some fixed law, which regulates and originates this development of morbid actions in, if not a frequent, at least an uniform mode of succession.

The Otaheitan eel (puhhe pirre rowte) produces, when eaten, a most copious scarlet eruption of the skin—most probably urticaria—and occasions *sudden tumefaction of the abdomen*, together with swelling of the extremities, hands and feet; the pain felt in the limbs is so excruciating that the patient becomes quite frantic.

I may remark here, that this and many other species of fish which act as poisons on the system, give rise very speedily to paralysis of the extremities. You will find in the *Edinburgh Medical and Surgical Journal*, vol. iv. p. 396, in an excellent review of Dr. Chisholm's work on the poison of fish, an account of the effects produced by eating the *Muræna conger*, the following passage: "In the course of the following night they were all seized with violent griping and cholera, together with a peculiar sensation of the lower extremities, attended with violent convulsive twitches, and faintings. They all perceived a brassy taste in the mouth, and a rawness of the œsophagus, as if it had been excoriated. These symptoms continued to afflict the negroes for a fortnight, and *then terminated in paralysis of the lower extremities.* After suffering for several months, they recovered with difficulty."

Werlhoff, as cited by my friend Dr. Autenrieth in a book * of extraordinary ability and research, gave a case where the *Gadus æglesinus asellus* produced a violent affection of the stomach and bowels, together with urticaria. Urticaria, diarrhœa, dysentery, paraplegia, are said, by the same author, to be frequently observed in consequence of eating the flesh of the *gray snapper.* Foster relates a similar train of accidents produced by eating the *Sparus pargus* (porgee). In short, I could bring forward citation after citation in proof of the truth above advanced; but I have done, for enough has been already said to establish the point in question.

Having established the fact that disease of the digestive organs is often intimately associated with urticaria, it remains to prove that a similar connexion exists between hepatitis—the cause of the derangement in the digestive organs in the case before us—and arthritis. Every one has observed how frequently inflammation of the joints becomes in its course complicated with inflammatory affections of internal viscera. In general those viscera whose component tissues are most similar to the articular are the organs affected. Hence the heart and pericardium are so often attacked in the course of rheumatic fevers. It sometimes happens, however, although less frequently, that the internal organ attacked has little analogy in point of tissue with the joints. Thus, in rheumatism and in gout, the stomach, the bowels, the lungs, or the liver may become engaged; and of these none, perhaps, so frequently as the liver. We need not be surprised at this, when we consider how intimately the digestive function is connected with arthritic inflammation, which is indeed generally preceded or accompanied by well-marked symptoms of hepatic and stomach complaints. Indeed almost all medicines that afford relief in arthritis are attended with well-marked symptoms of their having acted upon the secretions of the alimentary canal and liver. Thus colchicum seldom diminishes the pain and inflammation of the joints, until it produces copious bilious evacuations.

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* *Ueber das Gift der Fische:* Tubingen, 1833.

There is another sequence of disease, not unfrequently observed, but of which the connexion has not been hitherto noticed by any writer, as far as I can ascertain. About two years since I was consulted by an English gentleman, who had been ill for a considerable time. The history of his case from the commencement was this:—Three years previously he had venereal—used and abused mercury, was exposed to cold, and got periostitis. He now got into a bad state of health, used mercury a second time, obtained some relief, and then relapsed again; finally, after having used mercury three or four times, he was attacked with mercurial cachexy, became weak and emaciated; the periostitis degenerated into ostitis, producing superficial caries and nodes of a bad character; he had exfoliation of the bones of the cranium and rupia, and was reduced to a most miserable state. Under my care the symptoms gradually disappeared; he recovered to all appearance, and even got fat. He then caught cold, and relapsed again. At last his liver became engaged; he was attacked again with hypertrophy of the liver, ascites, and jaundice, and died soon afterwards.

Here, then, we have venereal, abuse of mercury, periostitic inflammation, abuse of mercury followed by exacerbation of the periostitis and establishment of mercurial cachexy; and the history of the case is wound up with hypertrophy of the liver. This was the first case in which I had observed this concatenation of diseases; since that period I have seen a similar train of morbid phenomena twice in private practice and once in hospital. First, we have abuse of mercury, then periostitic inflammation and mercurial cachexy, and the scene is closed by morbid enlargement of the liver. Now I do not look upon this sequence as merely fortuitous. The diseased actions are, I think, related as cause and effect, and each successive condition is consequent on the previous one.

It may not be amiss to mention here some curious circumstances observed in the case to which I have just alluded. While this gentleman's liver was enlarging, there was no tenderness of the right hypochondrium on pressure. I have observed the same absence of tenderness in all the cases of this description I have witnessed. The gentleman could bear pressure over the hepatic region without any inconvenience, and yet the liver was so enormously increased in size, that its inferior margin extended almost down to the pelvis. What is equally remarkable, he had no fever, and the tongue was perfectly clean and moist during the whole course of the hepatic affection. In my observations on a case in the fever ward, I remarked a few days since that some persons were too hasty in drawing inferences from the state of the tongue as to the existence of affections of the digestive organs. I shall not touch on this point, however, at present, and shall merely observe that this gentleman's tongue was perfectly clean and moist, notwithstanding the morbid condition and rapid growth of the liver. Another curious circumstance was that during the hepatic affection digestion appeared to go on very well, at least so far as the formation and due expulsion of feces are concerned. The alvine evacuations were regular, and the matter discharged presented the form and consistence of that which is passed by a person in good health. But there was a peculiarity in it to which my attention was first directed by the patient, who was an intelligent and observant person. The cylinder of fæcal matter was composed of parts differing in colour and appearance: two or three inches consisted of pale clay-coloured substance; and immediately after this another portion, of about the same length, was observed, presenting the ordinary bilious or brown colour of natural excrement: and then again another mass of clay-coloured matter, without any obvious trace of bile. This appearance I have now frequently witnessed; and the inference to be drawn from it is this,—that in such forms of hepatic disease the functions of the liver are performed, as it were, intermittently; it secretes bile during a certain period of the digestive process, then stops, and then secretes again.

This peculiarity is noticed in many diseases of the liver; and it is important to remark, in attempting to explain the *rationale* of these hepatic affections, that in no disease of the liver is this symptom more frequently observed than in the scrofulous. Scrofulous disease of the liver is that state in which there is an increase of size in the organ, with induration and imperfect secretion, but without any remarkable tenderness. This condition in children is accompanied by irritability of the digestive organs, fretfulness, emaciation, loss of sleep, and impaired nutrition. The little patient becomes what is termed "pot-bellied," and labours under thirst, debility, and febrile excitement. This has been frequently called remittent fever, and disease of the mesenteric glands, but in my opinion unjustly. It is only a form of general cachexy connected with the scrofulous diathesis, affecting secretion and nutrition in general, and the digestive and biliary systems in particular. It would be quite wrong to imagine that in this form of disease the liver is the cause of the whole train of morbid phenomena; it is merely affected in common with other organs, and forms only an individual feature in the group of symptoms.

Now, in this form of scrofulous cachexy, where you have diarrhœa, emaciation, fever, thirst, and restlessness, the liver is frequently affected in the manner already described; and in the loose stools of such a child you will find one part bilious, another part clay-coloured; they will be yellow this day, and pale the next, accordingly as the liver secretes bile or suspends its functions. But in this instance, I repeat that the liver is only one of many organs affected by the same general cachexy. Could we ascertain the derangements of other secreting organs with the same facility, it is very probable we should find similar evidences of the morbid influence which pervades the whole system.

This view of the question shows that you are not to expect to succeed in removing the disease by the use of calomel or any other mercurial preparation. Many of those persons whose practice is little better than routine, when called to treat a case of this description first examine or inquire as to the nature of the alvine evacuations, and fixing on the single symptom of deficiency of bile, immediately prescribe calomel to be repeated or continued until the secretion of the liver is established; but they forget that this state of the biliary system depends on the general state of health, and that the absence of the bile is the consequence and not the cause of the disease. Almost all the organs of the body are affected; and though calomel may restore the secretion of the liver for a time, it cannot bring back the organ to its natural state, or cure the disease. The malady is to be remedied in a different way: the secretions (and that of the liver among the rest) are to be improved by change of air, by an appropriate diet, by exercise, tepid or cold bathing, and the use of those remedies which are adapted to modify or correct that state of the system on which the general derangement depends.

An observation of such cases has led me to a train of reflection respecting the occurrence of the same order of symptoms in persons who have been injured by the abuse of mercury. Many persons who get venereal employ mercury injudiciously, and fall into what has been termed mercurial cachexy, in which there is a general unhealthy state of the organs. A patient who has fallen into this state very closely resembles a scrofulous person, and is apt to labour under the same emaciation, impaired nutrition, irritability, feverishness, and the same sort of cutaneous, glandular, and periostitic affections. The chronic mercurial cachexy is very like the scrofulous, and attacks very nearly the same organs and tissues. Hence the difficulty of curing affections of the liver and other organs when they are the result of this depraved habit. This is the key to the explanation of those horrible ravages which we frequently witness in cases of venereal disease complicated with mercurial cachexy—a state of constitution which is closely allied to the scrofulous. You will frequently meet with this consecutive affection of the liver in cases of morbus coxæ, where the patient has been labouring for years under ulceration of the joint. The growth of the rest of the body appears checked, the patient is stunted and emaciated, while the liver increases rapidly in size. It was from observing the occurrence of liver disease in persons labouring under the scrofulous cachexy, that my attention was first turned to its occurrence in persons broken down by long or injudicious courses of mercury.

One word, gentlemen, as to the curability of hepatic affections of this kind. I believe that it is always an unpromising form of disease; but persons of originally good constitution, and under the age of thirty, will generally escape if treated judiciously, and with proper care and attention. Some months ago I attended, with Sir Henry Marsh, a young gentleman labouring under this affection as a consequence of the abuse of mercury. We found him greatly emaciated, and labouring under considerable enlargement of the liver, with commencing ascites. He had also great determination of blood to the abdomen, diarrhœa, and hemorrhoids. By strict attention to his bowels, a well-regulated diet, change of air, and the use of taraxacum, conium, and hydriodate of potash, he was ultimately cured after an illness of nearly two years, during which the liver had grown to an enormous size. I may state that he is at present in good health, and that the liver is nearly reduced to its natural dimensions: this gentleman's age is about four-and-twenty.

I observed one circumstance in the progress of this case which is worth noting. He was suddenly attacked with a papular form of purpura, accompanied by much tingling and itchiness, and answering to the description given of *Purpura urticans.* This peculiar eruption was very troublesome at night, and formed several successive crops which altogether lasted a month. It occupied the extremities upper and lower, and was very abundant on the latter. The gentleman wore a bandage to relieve a varicose state of the veins of the left leg. Now the eruption never appeared in the parts subjected to the pressure of the bandage, although it was very thick immediately below and above these parts.

I may observe that it is entirely as the result of the cachectic habit, this enlargement of the liver which I have now been speaking of is observed. I have assumed this principle as the basis of my argument, and I think it is founded in fact and truth. It is also curious to observe that the same cachectic state which gives rise to emaciation and decay of the body generally occasions hypertrophy of some particular organ. What we most commonly observe in such conditions is general wasting of the system, accompanied by increased morbid nutrition in certain organs. This appears to be the general law. You perceive that in the explanation I have given, I have supposed that enlarged liver is the result of a general cachectic state of the system, and it is of importance to recollect that this state may be brought on by the injudicious exhibition of mercury, or by carrying mercurialization farther than the constitution will bear. In this instance we are compelled to allow that our practice may furnish weapons to be turned against us by the disciples of homëopathy. It cannot, however, be denied that the immoderate use of mercury has been productive of liver disease. The late Mr. Hewson pointed out this to the attention of those who visited the Lock Hospital while under his care. At this period it was the custom to salivate every patient, and keep him under the full mercurial influence for a month or two; and it frequently happened that, just as the mercurial course was finished, the patient got disease and enlargement of the liver. Were I inclined to theorize, I might perhaps offer some fanciful hypothesis in explanation of this occurrence, and might trace some connexion between the stimulant effects of mercury on the liver, and the subsequent hypertrophy. I shall, however, content myself at present with noticing the fact, and leave the explanation to my juniors, who always explain matters, according to my observation, much more readily than their seniors.

There are also other diseased states of the system in which we have enlargement and morbid alteration of the liver. I can point out to you four different states of the system in which hypertrophy and disease of the liver form one of the results of the general affection of the system. The next of these to which I shall direct your attention is scarlatina. Those who have attended the wards during the past month have seen examples of this. We have observed during the past week two patients labouring under scarlatina, who got disease of the liver and jaundice. One of the patients, a little boy, was attacked with the disease in an extremely violent form, accompanied by high fever and a very remarkable eruption. In a few hours after the exanthema appeared the entire cutaneous surface was dyed of a brilliant red; in fact, the skin looked as if it had been painted over, and there was not a single spot free. In cases of this kind the violence of the cutaneous inflammation is sufficient to kill, without any other unfavourable complication; and the patient seldom lives more than three or four days. You observed in this case the whole epidermis peeled off. But what I wish to direct your attention to is, that this boy after two days had evident symptoms of disease and enlargement of the liver. A young man in the same ward had also an attack of scarlatina, but in a milder form. On the third day he likewise got inflammation of the liver, but was cured by general and local antiphlogistic treatment.

In a previous lecture I have explained to you that scarlatina is one of those diseases in which a train of unfavourable sequelæ are apt to remain after the removal of the original complaint. Persons after recovering from the exanthematous fever will sometimes get into a bad state of health, and instead of convalescing, become restless and feverish towards evening, have an irritable jerking pulse, hot skin, derangement of the digestive organs, diminished urinary secretion, and finally become dropsical. Now, from observing the supervention of hepatic disease in such cases, both in hospital and private practice, my attention has been directed to the liver; and I never omit making an examination of that organ when called to treat those symptoms which are looked upon as the sequelæ of scarlatina. In many of these patients I have found the liver in a state of inflammation of rather a chronic character, and without any of that remarkable pain or tenderness which characterizes acute hepatitis. But still it was inflamed, as proved by the benefit derived from local antiphlogistic means; and, moreover, its condition appeared to retard and prevent convalescence.

Not long since a friend of mine, a very intelligent practitioner, who was attending a case of this description, and had tried a variety of remedies without any benefit, was very much surprised when I drew down the bed-clothes and showed him that the liver was diseased. He had not thought of the existence of anything like hepatic affection, and was very much surprised that his treatment had proved so ineffectual. By the use of leeches to the right hypochondrium, the employment of mercury, and a proper regulation of diet, the patient was soon relieved, and the fever, thirst, and anasarca quickly disappeared. In cases of this kind the hepatic affection is the result of the general inflammatory diathesis superinduced by scarlatina. You are all aware that nothing is more common after scarlatina than inflammation of various organs. Thus some persons are attacked with pleuritis, some with pneumonia, others with inflammation of the liver. Many persons continue in a valetudinary state after the eruption had declined; they do not convalesce according to our expectations; the pulse remains rather quicker than natural; the bowels are deranged; the appetite bad; thirst urgent; and urine scanty. In many of these cases you will find that there is a species of chronic hepatitis going on, which keeps up the feverishness, and retards convalescence. This is a point of great importance, to which I am the more anxious to draw your attention, because even the latest writers on scarlatina have either entirely omitted or very insufficiently noticed it.

There is another organ whose morbid affections frequently implicate the liver; I allude here to the heart. I have already spoken of certain cachectic states, in which the liver becomes enlarged and hypertrophied as the result of the general derangement of the system. In the present case the hypertrophy and disease of the liver originate in a morbid condition of the heart; this is a very frequent cause of hepatic derangement. You have an example of it at present in the chronic ward, in the case of a poor man labouring under bronchitis of long standing, with disease of the heart, dropsy, and enlargement of the liver. In cases of this description it is a matter of some difficulty to determine in what organ the morbid sequence commences; for where many diseases co-exist, it is not easy to ascertain how they are related to each other as cause and effect. I have, however, had several opportunities of observing the progress of the disease from the commencement, and the manner in which the different organs become successively implicated.

Some time ago there occurred a remarkable example of this form of hepatic affection in a relative of mine, aged 14, who, in consequence of exposure to cold, was attacked with rheumatic inflammation of the joints of a very intense character. Owing to a want of proper care, the disease was allowed to go on unchecked, and metastasis to the pericardium took place. I happened to be out of town at the time, and he had no advice or assistance for nearly twenty-four hours. Pericarditis of a violent character became developed, and it was only by the most energetic treatment that he escaped with his life. He had pericarditis with effusion, and all the physical signs and symptoms of carditis. After the acute symptoms were removed, the signs of adhesion of the pericardium, hypertrophy, and partial valvular disease continued; and for a long time the heart's action was invariably accompanied by a loud bruit de soufflet. These affections were followed by dyspnœa and increased action of the heart. But this was not all. He next got inflammation of the testicle, and finally chronic hepatitis with enlargement. The liver grew to a very considerable size; it continued to enlarge for about seven months; and altogether he laboured under a chronic form of hepatitis for more than a year. At last the disease yielded to treatment, and he recovered completely.

This you will say was a fortunate termination; but in young persons the powers of nature often act in a very remarkable manner in remedying or removing disease, and cures are sometimes effected in such patients which it would be quite absurd to expect in persons advanced in life. After having laboured under a long train of diseases, and having continued an invalid for nearly five years, this young gentleman at last, owing to his youth and favourable constitution, surmounted all his maladies, and is at present as strong and healthy as any person I am acquainted with. In this instance the chronic hepatitis was the result of the pericarditis which formed the first link in the chain; and for the space of a year this young gentleman continued to labour under an affection of the liver, the result of disease commencing in the heart. This is a morbid sequence very frequently observed. You have pericarditis accompanied by inflammation of the lining membrane of the heart, partial disease of the valves, hypertrophy of the muscular substance, and then enlargement and induration of the liver. This is a very common complication, and deserves your most particular attention. When you see a patient whose appearance indicates disease of the heart—who has swelling of the face, dyspnœa, lividity of the lips, and turgescence of the cutaneous vessels—in fact, that peculiar expression of countenance which at once informs the practised observer that the patient is labouring under disease of the heart, you should not neglect to inquire after the condition of the liver, for in such cases it is very frequently in a state of chronic disease. I pointed out this circumstance some time since in the case of a late surgeon, Mr. M., and directed the attention of the medical gentlemen engaged in the treatment of the case to the liver, in which no one had suspected the existence of disease. Recollect, therefore, that in many cases of disease of the heart you will also, on examination, find disease of the liver produced, as far as I can judge, in the majority of instances by disease of the heart; at least, I think I have never seen any case in which the hepatic affection had the initiative, and seemed to have brought on the organic affection of the heart. In Mr. M.'s case and several others which I had an opportunity of watching from the commencement, I have no doubt that the disease of the liver was secondary, and that the morbid sequence commenced with the heart. I am quite convinced that disease of the liver may give rise to a functional derangement of the heart; for whatever impairs secretion and deranges digestion will give rise to palpitations, tendency to syncope, and other phenomena of functional disease of the heart; but I have never seen any example of organic disease of the heart as the result of disease of the liver.

It is of some importance to be aware of this complication; for in treating the disease of the heart you must also attend to the hepatic affection, because it has a tendency to aggravate and confirm the cardiac symptoms. This affection, however, is not to be looked upon as acute, or even subacute hepatitis. There is scarcely any pain of the side or tenderness present, and the patient is not always jaundiced; it appears to be scarcely anything more than congestion, causing hypertrophy and chronic morbid growth. I shall not, however, speak too positively on the subject, as the difference between hypertrophy and inflammation of a low and obscure character cannot be easily determined. I am glad to find that the subject I am now discussing has been taken up by so able an observer as Dr. Bright, who, in the third number of *Guy's Hospital Reports*, p. 605, has made some excellent remarks on the influence of heart disease in producing congestion of the liver.

There is another disease in which derangement of the liver is a common symptom, and I bring it forward chiefly for the purpose of rendering the subject under discussion more complete, as it is an occurrence well known to the practitioners, and sufficiently dwelt on in medical books. I allude to that affection of the liver which is observed in cases of intermittent fever. Ague frequently produces a powerful determination to internal organs, particularly the liver and spleen, and if treated badly or unsuccessfully, is apt to bring on disease of the liver. The organ becomes congested, hypertrophied, and indurated, and presents a condition somewhat analogous to that which supervenes on disease of the heart, or results from the cachectic state of constitution produced by mercury or scrofula.

The next form of organic derangement which I shall briefly touch on is that of the spleen. It is of advantage to place cognate affections beside each other for the purpose of comparison; by doing so we frequently derive many instructive and useful analogies. Besides, we have had a remarkable case of enlargement of the spleen in our wards at the same time we had the cases of hepatic disease to which I have alluded.

The circumstances under which enlargement of the spleen takes place differ in many points from those which determine hypertrophy of the liver. We have but few examples of inflammation of the spleen, while the cases in which enlargement and congestion of that organ take place are numerous. From the peculiarity of its anatomical structure, the spleen is very apt to become suddenly enlarged. Like the liver, it may become indurated and hypertrophied from intermittent or from some general disease affecting the system, and thus lead to a train of secondary phenomena, the most remarkable of which is dropsy. But there is one peculiar symptom attending enlargement of the spleen, which I have frequently pointed out to the attention of the class as observed at least in two-thirds of the cases, and of which we had an excellent specimen in the patient under treatment in the chronic ward.

The history of this symptom is the more curious as showing a remarkable uniformity in the phenomena of a peculiar disease at very distant periods of time. This is seen by comparing the most recent descriptions of Indian splenitis, as given in an able analysis of Voight's work on the spleen, in the *British and Foreign Medical Review*, and the description of enlargement and disease of the spleen given by Aretæus. The ancients, it is true, cannot be now considered as authorities to be followed either in pathology or practice; for they were ignorant of many of the most important facts connected with the healthy and diseased states of the human body. In consequence of their inaccurate anatomical notions, they were unable to appreciate or describe many of those details which now enrich the domain of pathological anatomy; their writings, however, are invaluable in many respects, as containing admirable descriptions of disease which still continue to affect the human body, and as recording certain groups of symptoms which are still associated. A comparison of their descriptions with those of modern times cannot fail to be extremely curious, and may even prove highly instructive; for if we find that certain internal affections have, from the most remote antiquity to the present period, been generally accompanied by peculiar derangements of distant parts, we are authorized in considering this connexion to be something more than accidental, and consequently we may be led to discover relations between organs generally believed to be quite unconnected with each other.

Thus, some time since I had three patients in succession under my care, who laboured under chronic enlargement of the spleen, who were all affected with a similar sort of cachexy, and had all the same affection of the skin—namely, superficial ulceration of the legs. This coincidence forcibly arrested my attention, and I was still more struck with the observation on finding that Aretæus had noticed this very circumstance in his admirable description of splenitis. "If (says he) the spleen does not suppurate, but becomes chronically enlarged, then the patients lose their appetite and become cachectic, swollen, and of an unnatural colour, while the surface of the body manifests a disposition to ulcerate, particularly on the legs: the ulcers are hollow, round, livid, sanious, and difficult to heal." This description agrees precisely with the cases to which I have already referred, and it coincides in a very remarkable manner with the account lately given by Dr. Voight, of chronic disease of the spleen as it occurs in India. He observes that the cachexy connected with the Splenalgia Bengalensis freqently manifests itself by a tendency to ulceration; the disposition to which is so great, that leech bites and blisters occasionally give rise to foul or phagedenic ulcers, which under certain circumstances, as where the patient has used mercury and is residing in a swampy district, will sometimes run on to a fatal termination. It is also curious that the predisposing causes of the different varieties of chronic enlargement of the spleen, as given by Voight, are exactly the same as those detailed by Aretæus; and both writers correspond in their statements as to the age and habits of life of persons most liable to this disease, as well as the nature of the locality and the season of the year most favourable to its production. This agreement between authors separated from each other by so many centuries, and who describe the disease as it occurred in different regions and among different races of mankind, is extremely curious, and exhibits a very remarkable example of the identity of the morbid phenomena produced by the same causes.

From the observations I have made in this lecture, you must perceive the advantage the physician gains from a knowledge of this connexion between the diseases of different organs, how much precision it adds to his practice, and what facility it gives prognosis. Additional investigations are much wanted on this subject; but based, as to prove useful they must be, on the accumulation of facts derived from experience, much difficulty lies in the way of their being undertaken.

LECTURE XXXI.

GOUT.

I SHALL in the present lecture make a few remarks on certain varieties of gout, of which I have recently seen several singular examples, premising some observations on constitutional inflammation in general.

There is no proposition in pathology better established than that there exist several constitutional affections capable of generating and modifying local inflammatory action; and that local inflammations, depending on a constitutional cause, are subject to very different laws from those which regulate the phenomena of common inflammation.

Another fact of equal importance in many points of view is, that local inflammations depending on a constitutional cause differ remarkably from each other, and in general present specific characters easily recognized. Thus, local affections arising from scrofula are not likely to be confounded with those depending on gout or rheumatism, and the inflammations produced by syphilis and other animal poisons exhibit peculiarities by which their respective origin and nature may be satisfactorily ascertained. It must, however, be admitted that, although advanced considerably in our knowledge of the phenomena of local disease depending on a constitutional cause, the subject still displays a wide field for investigation, and many points of much importance in pathology and practice require still further investigation.

Professor Cayol, in his Leçons Orales, has made some observations on this subject well worthy of attention. Speaking of the dependence of local disease on constitutional causes, he says, "Il faut nécessairement conclure que les dégénérations organiques ne sont pas cause, mais effet. Et dès lors, nous sommes fondés à vous dire, qu'au lieu d'user votre vie à chercher toujours quelles sont les dégénérations organiques et les altérations de texture qui *produisent* les symptômes des maladies, il serait bien temps de s'inquiéter un peu de savoir ce que *produit* ces dégénérations elles mêmes, en étudiant serieusement les caractères, la marche, et la tendance des acts vitaux qui les préparent, et qui les *produisent* réelment."

There is one fact connected with local inflammation depending on a constitutional cause not sufficiently noticed, namely, that certain affections of this kind are sometimes remarkably fugitive and transient. We are accustomed to regard the process of inflammation, whether common or specific, as one which generally lasts for some days; but it occasionally happens, that a peculiar diathesis will give rise to local affections having the characters of inflammation, and which run their course and terminate in the space of a few hours. This observation, which should be borne in mind in the investigation of diseases connected with the general habit, will serve to explain some of the anomalies which strike us occasionally in the study of constitutional maladies.

The first instance of this kind that came under my notice occurred in the case of a florid healthy-looking boy, aged six years, in whom, on attentive examination, I was led to suspect the existence of a scrofulous taint. At the time I saw him he was subject to a sudden and rapid formation of bumps, or tumors, on various parts of his body—sometimes on his arms, sometimes on his legs, and occasionally on the trunk. These circumscribed tumefactions were accompanied by a feeling of heat and tenderness, and apparently depended on local congestion, or effusion in the subcutaneous cellular tissue. But what was most remarkable in them was, they arose, ran through their course, and terminated in the space of four or five hours; they were suddenly developed, and disappeared with equal rapidity. In the course of a month, other more permanent inflammations were set up; scrofulous ophthalmia, glandular swellings, and ulcers supervened; the joints became affected, and the boy died in about a year and a half, with all the characteristic marks of the scrofulous diathesis. I have detailed this case before, and shall not dwell on it any further at present; but it is well worthy of notice, in consequence of the very brief duration of the first local symptoms.

Gout is another disease which occasionally exhibits examples of its peculiar inflammation attacking various parts and tissues of the body, and that for an extremely short period of time. It is well known that persons of a gouty habit are subject to sudden pains or twitches, which last only for a few minutes, or even seconds. I shall not stop here to consider what may be the nature of these fugitive pains; I may observe, that certain facts seem to prove that these pains are the result of a momentary congestion. Thus, in various neuralgic affections, and in inflammatory diseases in which the nerves are considerably engaged, pain is suddenly produced by coughing. If a man labours under neuralgia of the frontal or facial nerves, or if he is affected with sciatica, how are his sufferings increased when he has unfortunately at the same time a cough! Every time he coughs, the affected nerve gives notice that it feels the congestion by a sudden pain. Now, the only way in which coughing can increase a local pain is by favouring local congestion; that it is capable of doing this is proved by the redness of the face it occasions, as also by the hemorrhage from the nose, or from recent wounds, which is so often produced by a fit of coughing.

As there can be no doubt, then, that a momentary congestion may produce a momentary pain, we may infer that in many instances gouty twitches are owing to some cause which determines an instantaneous congestion of the affected part. Sometimes the congestion is more lasting, and the pain is proportionally intense and persistent. Thus, the late Mr. Daly, of Henry Street, mentioned to me the case of a gentleman, the lobe of whose ear was sometimes attacked suddenly by gouty congestion, accompanied by agonizing pain, but which never lasted more than a few hours. And I have myself recently suffered from a similar attack in the *cartilage* of the ear, which did not last longer than an hour, disappearing on the occurrence of gouty pains in the fingers.

This fact brings to my mind a curious case which some years ago came under the notice of Sir Philip Crampton, Mr. O'Ferral, and myself. A young gentleman of fortune perceived that the pendent lobes or tips of his ears were becoming elongated; they increased gradually in such a manner that he considered himself disfigured by their unseemly length, and therefore attempted their concealment by allowing his hair to grow in long curls, so as to hide the ears. This gentleman soon afterwards became dropsical and died; and, on dissection, Mr. O'Ferral found his liver in a state of fatty degeneration. On slitting up the elongated portion of the ears, he discovered that their hypertrophy had been occasioned by the deposition of a large quantity of fat. The subcutaneous adipose tissue and the omentum were likewise much loaded with fat. This observation is of much importance, as teaching us that fatty degeneration may be the consequence of a general tendency in the system to manufacture and deposit fat in the textures of different organs. In this point of view the change of structure in the liver must be regarded as an effect, and not as a cause, of the general derangement of the system and the fatal termination of the case.

One of the most remarkable instances of fugitive inflammation affecting various parts of the body, which has come under my notice, occurred in the person of a gentleman lately under my care. I shall not go through the whole history of his disease, of which he has favoured me with a very minute account, but shall merely state that he is of a gouty habit, has had an attack of gout in the stomach, and is at present subject to a gouty affection of a very extraordinary character. After labouring for some time under languor and weakness, accompanied by spasms, pain, and sense of weight in the stomach, the pain of the stomach ceases, and his face begins to swell at various points, generally commencing on the forehead, and involving the cheek and eye so as to close up the latter. He first feels as if a small current of air was directed on the face; then, as it were the fillip of a finger, or the bite of a gnat; and on looking in the glass, he suddenly perceives a tumor rising on the forehead, which, in the space of half an hour, becomes as large as a pigeon's egg, and, as he expresses it, moves down until it closes the eye. Sometimes it attacks his lips, and other parts of his face, but never affects his nose. These tumors have also appeared on various parts of his body; and he observes in his letter to me, that he is sometimes led to think that they attack his stomach also. Before and during an attack of the face, which generally occurs on the left side, the discharge from the nostril of the affected side ceases.

But what is chiefly remarkable in this case is the singular character of the local affection. The tumors arise, run through their course, and disappear in the space of a few hours, and on the following day there is no trace of their existence. Sometimes the lips inside of the mouth, palate, and uvula are attacked, giving rise to a very considerable inconvenience. Were such tumors to occur in the neighbourhood of the glottis, I need not say that they would be pregnant with danger of no ordinary character. I may observe that this gentleman has derived great benefit from the use of hydriodate of potash, and from decoction of sarsaparilla with nitric acid, and that his health is at present much improved. His case presents a very curious example of transient local inflammation depending on the gouty diathesis.

Having touched on the subject of anomalous local affections as connected with the gouty habit, I may here refer to a very singular affection of the teeth which I have observed in individuals of a gouty diathesis. The disease I am about to describe, though very singular and remarkable, has not been noticed by practical writers. A few preliminary remarks on the functions of the dental nerves appear necessary, in order to enable you to form a more exact idea of its nature.

The teeth are immovably fixed in the jaws, and consequently require no nerves of motion so far as they themselves are concerned; they are on the other hand abundantly supplied with nerves derived from the fifth pair—a nerve of sensation, and their nervous apparatus is developed and expanded within their substance in a manner which shows that nature has bestowed a greater degree of care on this than on any portion of the nerves destined to perform the office of touch. In this respect, they, to a certain extent, approach the affection of the nervous apparatus of the organs of sense properly so called. In truth, no part of the mechanism of the human body seems more admirable than that which thus associates together in function a soft nervous pulp and a solid osseous substance; and associated together they assuredly are, for the teeth, though encrusted with a coat of enamel as hard as steel, are very delicate organs of touch; the most minute bodies when hard may be distinctly felt if placed between their edges; and matters of more yielding texture, as a leaf of paper, or a rose leaf, can be distinguished in the same position.

The delicacy of touch enjoyed by the teeth has not attracted due notice, nor have its uses been sufficiently dwelt on, for to this sense are owing the ease and precision with which, as instruments, they perform their proper office of cutting, tearing, and grinding the food. It is from the feeling imparted to their edges that we derive instant knowledge of the situation, and many of the physical properties of the morsel, such as its hardness, consistence, shape, size, &c., in consequence of which it is either at once submitted to the action of the teeth, or is removed to be placed in another part of the mouth, and in a more convenient position where teeth of a different shape and form may be brought to bear on it. Without this exquisite sense of feeling, one row of teeth could not act in concert with the other; the incisors and molars in the under could not adapt their cutting and grinding surfaces to those in the upper jaw, nor could certain information be conveyed to the muscles of the lower jaw for the purpose of commanding the consecutive motions they are called on to perform.

In fact, the teeth are not merely cutting instruments, but are endowed as it were with intelligence; they are, it is true, assisted in ascertaining the size, portion, hardness, and other physical qualities of the morsel by the tongue and cheeks, but they perform besides a peculiar function, that of feeling the intimate texture of what is submitted to their immediate operations, thereby warning us instantaneously when the morsel contains anything detrimental to their own substance; without this sense of touch how soon would our teeth be chipped away and worn by minute but hard matters, as grains of sand, which no care can entirely exclude from our food, but which the teeth detect at once when in contact with their edges, and which they refuse at once to act on. In truth the teeth may, in this point of view, be considered as a sort of fingers fixed within the mouth, destined to feel, examine, and adjust the morsel preparatory to placing it in the position most favourable for mastication.

It is very strange that no example of paralysis of the dental nerves has as yet been observed. This subject has engaged my attention for several years, and I have been in the habit of inquiring from all my paralytic patients whether the sensibility of the teeth was lessened; but in no one instance have I been able to detect anything approaching to the loss of sensation in these organs, an immunity difficult to account for, and I believe unexampled, for I am not aware of any other nerve either of sense or of motion which is not occasionally involved in the progress of paralytic affections; nay, I have been more than once been obliged to direct the removal of teeth in hemiplegic persons in consequence of toothache on the paralytic side. This immunity from paralysis, corroborated by the extensive experience of Mr. M'Clean, seems the more surprising, when we recollect how subject the dental nerves are to the opposite affection, or a morbidly increased and exalted state of sensibility constituting the various forms of toothache.

Some physiologists have been inclined to suppose that the temperature of bodies is judged of by other nerves than those which are the instruments of the sense of touch; but it appears that if other arguments against this hypothesis are wanting, the instance of the teeth alone would be sufficient, for here most undoubtedly the sense of touch and the discrimination of temperatures are both functions of one and the same nerve, for the teeth possess but one.

The disease to which I would now direct attention consists in an insuperable desire on the part of the patient to grind his teeth. This desire originates in a disagreeable uneasy sensation in the teeth themselves, and is for the moment alleviated by forcibly grinding them together, but immediately returns when the patient ceases to perform this action, which is therefore continued when the disease is confirmed during the entire day. When asleep the patients no longer grind their teeth, the grinding being in all cases the result of voluntary motion. I have now become acquainted with the cases of four persons so affected, and it is very remarkable that they were all of a confirmed gouty habit. The first person in whom I observed it was my late excellent and esteemed friend the Countess of Egmont, in whom this habit had become so confirmed, that she was impelled to indulge in it continually, for the moment she desisted, the uneasy sensation in the teeth became insupportable, and consequently she was obliged to give up all society for several years before her death. The grinding was in her case strong and forcible, and having been so long continued, at last wore down her teeth to the very sockets. I consulted several of the most eminent surgeons in London on her disease, among the rest Mr. Abernethy, but none were able to suggest any means for its alleviation. She was so thoroughly convinced that some permanent cause of irritation existed in the teeth themselves, that at different times she had several of them drawn in hopes of procuring relief, but they were found to be perfectly sound.

I was lately consulted by the Rev. Mr. B., likewise of a gouty habit, and who is driven from general society by precisely the same affection. In him the molar teeth are worn quite flat and smooth, and the incisors and canine teeth have undergone a remarkable change, particularly the former, which being constantly *whetted* by each other, have acquired chisel-shaped edges, and are so sharp that when he inadvertently passes his tongue over them, they make an incised wound like that inflicted by a sharp knife. This gentleman's teeth have the enamel all worn off the crowns, and consequently their surfaces present a section of the internal or osseous portion of the tooth; and it is remarkable that in this as well as in the other cases, the internal or nervous cavity of the tooth is never exposed, but appears to be filled up with bony matter, in proportion as the process of grinding wears away the crown, just as has been observed in the case of old men, such as sailors, who have been in the habit for many years of chewing sea-biscuit. The same phenomenon has been likewise observed in the teeth of skulls supposed to have been Roman, from which it has been inferred that they had generally subsisted on very hard food.

The third case was that of a young clergyman in the south of Ireland, likewise of a gouty habit, and who was afflicted with *tic douloureux* of several branches of the fifth pair, and, among the rest, of the dental nerves of the left side; in him the teeth in the left side only were ground down, and the disease ceased after a continuance of two years.

The third case I have not seen, but the following particulars have been furnished me by Dr. Battersby:—

"Henry W., County Meath, aged 60, has suffered from attacks of gout for the last thirty years, which are now so tedious and severe as to confine him to his bed for at least five months annually; about three years ago he was observed gradually to get a habit of grinding his teeth, which he now does constantly while awake, and so loudly as to be heard in the next room; he is not conscious of it unless when spoken to, I believe, and his teeth are quite ground down. Two years ago he had an attack of what he called gout in his teeth, and wanted to have them pulled out."

I have now seen several cases of this kind, and I have observed that they all occurred in persons of the gouty diathesis. The grinding of the teeth continues for years as a daily habit, and produces very remarkable changes in the conformation of these organs, affecting sometimes one side of the jaw, sometimes both; so that in confirmed cases we frequently find the teeth ground down to the level of the gums. There is not at present the slightest doubt on my mind that the irritable state of the dental nerves which gives rise to this irresistible tendency to grind the teeth, depends chiefly on the existence of gout in the constitution. I may observe, however, that in many persons in whom the teeth are found worn nearly to the gums, there appears to be another cause in operation. Thus, in cases of indigestion, it is not unusual to find the enamel of the teeth partially or considerably worn away long before the natural time; and in such instances we used formerly to attribute the injury to the generation of acids in the stomach. The researches of Donné and Thomson, however, have shown that the saliva is subject to very remarkable alterations in certain forms of dyspepsia, and that whenever the disease is accompanied by much irritation of the gastric mucous membrane, and derangement of its secreting functions, the saliva becomes extremely acid, and, of course, capable of corroding the enamel of the teeth. The following case has recently come under the notice of Mr. Pakenham, of Henry Street:—

"A gentleman, aged 45, slightly made, but muscular, and born of healthy parents, was attacked with shivering and loss of power of the right side after a severe wetting. He recovered under appropriate treatment; but, about a year afterwards, began to observe in himself a tendency to grind his teeth, which gradually increased to such an extent as to prove a nuisance to himself and every one about him. Under these circumstances he consulted an eminent surgeon in Dublin, who applied the actual cautery behind one of his ears, slightly affected his system with mercury, and extracted one of his teeth,—all with considerable relief, which lasted for about six months. He then became as bad as ever, and applied to another surgeon, who tried iron in every form without success: and subsequently to a third practitioner, who used in addition leeching, blistering, postulation with tartar emetic, and various other remedies, but without any favourable result. All this time his medical attendants, so far from suspecting the presence of gout, ridiculed the idea of its existence.

"About three months ago this gentleman came to Dublin, went to dine at the house of a friend, and, with some others, supped late at night, and drank some whisky punch. Next day he had vomiting, purging, and epigastric tenderness, and on the day after, the ball of his great toe became swollen, hot, and exquisitely painful, leaving no doubt as to the nature of the affection. In this gentleman's case the grinding of the teeth is not constant, but it is always greatest when the stomach is most deranged. The teeth in the under jaw are all sound: three or four of the molars of the upper jaw have been extracted. The four upper incisors are ground nearly half way through to the gum on the one side, while the lower are very little worn. By pressing the tongue against the upper incisors, or by touching a certain point of one particular tooth, he can at any time arrest the tendency to grind, and can suspend it as long as pressure is continued in the manner just described."

Although I have as yet been unable to discover any mode of alleviating the sufferings of patients affected with this hitherto undescribed disease, I have thought it right to give you the preceding short account of its chief symptoms, in the hope that others may be induced to publish the results of some successful method of treatment.

With the view of further illustrating the varieties of gout, I shall detail the following remarkable case, which came recently under my notice. The patient, a gentleman of large fortune, is of a strong and athletic frame, about five and thirty years of age, and a member of a family subject to gout. He was much addicted to field sports, and accustomed, in cold weather, to frequent immersion of his feet in cold water, in pursuit of his favourite amusement, snipe-shooting. The consequence of this exposure has been, that he has been labouring for some time under a neuralgic affection of the lower extremities, which commenced in the feet and ankles, and extended gradually upwards, involving the whole of the lower extremities as far as the hips, and giving rise to sufferings of a very intense character.

Repeated exposure of the feet to cold seems often to lay the foundation of creeping paralysis. Now in this case there is some danger that the gentleman, were proper measures neglected, may ultimately become paraplegic, or even generally paralytic. I do not bring this case forward as an example of gouty pains gradually advancing from the extremities towards the spine; for although

I strongly incline to the opinion that his complaint is of a gouty nature, and although most of his medical advisers have suspected a gouty complication, still this is by no means a decided point; but the opinion of his usual attendant, Dr. Little, of Sligo, exactly agrees with mine, as he considers the case to be gouty neuralgia. Be this as it may, his case presents a very interesting specimen of creeping neuralgia, chiefly affecting the cutaneous nerves (nerves exclusively destined to perform the function of sensation), but gradually implicating the nerves of motion in the disease. I shall now proceed to lay before you the details of this case, which have been noted with singular accuracy and ability by the gentleman himself. In a letter to me he observes:—

"As you wish for a description in writing of the manner in which I am affected, I subjoin every particular I can think of which seems likely to throw any light on the subject.

"It is now nearly five years since I began to suffer severely from pains in my limbs, which for the last two or three years I have looked upon as neuralgia. About a year previous to that time I had occasional pains in one foot, which increased so as to become violent on one occasion, after a long ride. I had, however, been always in the habit of riding, and considered that exercise to agree particularly well with my health. Indeed, I had found hunting of great use to me when suffering from liver complaint, having had inflammation of the liver twice in my life. It is now fourteen years since I had the last attack of liver disease, and I very seldom have pain in my side; whenever it occurs, it is generally removed by the use of a little blue pill.

"When first the pains in my limbs commenced, they were confined to my feet; then, for a long time, extended no higher than my knees; latterly they had ascended as far as my hips, where, and in the groin, I sometimes experience great suffering. I have had occasional twitches in my arms, and very slightly across the chest. The pain always comes on with sudden violence, which renders it very hard to bear, especially when it attacks me during sleep. I am frequently aware of its approach, from a general feeling of discomfort and depression; from which, in the beginning of my complaint, I used to suffer very much for two or three days before an attack. These paroxysms have, for four years, shown a great tendency to periodicity, recurring generally once every week, commencing on Saturday or Sunday, and sometimes on Friday, and lasting till Monday. They have twice or thrice lasted for a week together, but sometimes continued only a few hours.

"In the commencement I have occasionally been free from them for two or three months together; and within the last year was free from them, at two different periods, for a whole month. When in pain, I have never experienced the slightest alleviation from anything, except at times from a full meal with wine, particularly champagne. I have often been unable to remain in bed from the violence of the pain, which is increased by the weight of the bed-clothes, or the slightest touch of anything; even the air blowing on the part brings on violent torture: at the same time I can bear strong pressure, or even a blow on the parts, without making me worse. The pain appears to be quite on the surface, except that sometimes it appears deeply seated, particularly in the ankle-joint and shin-bone. It is unaccompanied by any redness or swelling, and flies instantaneously from one limb to the other, rarely occurring in both at the same time. It leaves behind great weakness of the affected limb, so as to oblige me to walk with a stick for some time, and occasionally with two.

"One very unpleasant consequence of the pains in my limbs is, that I now find I cannot use exercise on horseback, if I leave it off for any time. I have found this and walking at all times conducive to my general health. Indeed I can still walk a good deal, even during an attack, although it is very painful, particularly when setting out. I find it necessary almost constantly to have recourse to aperient medicine—generally rhubarb pill. At times I have had giddiness of my head, and noise in my ears, to a very distressing degree; and have had recourse to powerful purgatives, and even bleeding, to remove the symptoms, without effect. A medicine, principally nervous, in which gentian was an ingredient, relieved me at one time, after finding the above remedies ineffectual.

"I have already tried iron, mercury, nitro-muriatic acid, stramonium, arsenic, and the external use of croton oil, without benefit, except that I felt rather better for a month after the use of these remedies, but no longer, and the pain returned with great violence at the end of that period. The counter-irritation appeared to increase my sufferings. I have also tried anodyne embrocations without effect. Anxiety of mind, or annoyance, often brings on an attack. I even remarked, the other day, that it came on instantaneously, on breaking a tooth whilst eating. On the other hand, excitement, whether from a sudden necessity for exertion, as on occasion of an accident, or anything that gives a pleasing interest and occupation to my mind, such as travelling through an interesting country, seems to keep off, and sometimes even remove an attack."

The following most interesting account of his own case, which I received from Dr. Mackness, of Hastings, and which I read from his letter, bears much similarity to the foregoing case; on which account I introduce it here. All the symptoms, however, may, I think, be ascribed to *functional* derangement of the spinal marrow:—

"The symptoms in the case of what is here called gouty neuralgia are, in some respects, very similar to what I have myself suffered, and this without any hereditary or constitutional tendency to gout. I am inclined to think that the malady has its origin in slight inflammation or irritation of the spinal cord or its membranes, this state being excited by certain impressions made upon the extremities of the nerves, especially of the lower limbs, and carried along the trunks to the nervous centres,—cold being usually the exciting cause; but for this to produce the specific effect of which I speak, I believe it necessary that the digestive organs should be in a state of irritation: such was the case with me. I was residing in the country at the earliest commencement of my disease, where the atmosphere was usually loaded with moisture, arising from a sluggishly flowing river, a short distance from the banks of which my residence stood. I had, in attending to my professional duties, much night work; riding, perhaps, for several miles on horseback through a foggy atmosphere, and then having to sit for hours in a cold cottage or farm-house, my feet and legs as cold as if they were immersed in ice water. I was very temperate in my habits, but I suffered much from dyspepsia; at first the pains were slight, and the paroxysms very short, but gradually they became more severe and of longer continuance, generally affecting the lower extremities; at the same time there was slight loss of power in these limbs, which manifested itself by a little awkwardness of gait, and was more observable to my friends than to myself.

This state of things continued rather increasing in severity for two or three years, at the end of which period my gait became much more unsteady, and I found it difficult to walk in the dark, or where my eyes were not fixed on the road. Bilious attacks, attended with constant sickness and vomitings of bile, with severe pain in the brow and shoulder, then began to visit me at stated intervals, these intervals becoming shorter and shorter, until I rarely passed a month without having had two or three. In the meantime the pain became more severe; so intense was it at times, that I have as much dreaded any of my family coming within a yard or two of me, for fear that some part of their dress might touch me, and thus excite a paroxysm, as any hydrophobic patient dreads the sight of fluid or any glistening surface. A draught of air was often quite sufficient to excite the paroxysm: what was perhaps worse to bear than even the pain itself, was the constant dread I had during an attack of its coming on. It was not one part only that was affected, but oftentimes the whole of the extremities in turn, yet mostly the lower. I have sometimes tried to point out to my friends the spots which the pain attacked, but so quick were the transitions from one place to another, that although I have tried to touch each part successively, I have always failed in being able to do so sufficiently quickly. The cutaneous nerves were often so sensitive, that the slightest touch would produce the most exquisite torture: thus giving an example of the law established by Dr. Marshall Hall, that in proportion as the muscles become less under the control of the will, this irritability becomes increased. This continued strain upon the nervous system produced epileptic fits, which continued for some years, and another affection of the nervous system—spasmodic closure of the glottis—began at this time to show itself, often threatening suffocation. It is very singular that my father was subject to the same affection. At length I gave up my professional duties, after having suffered for four years a martyrdom, and went abroad, at first with some benefit, but I afterwards became worse. Having been accustomed to a very active life, the change to one of complete idleness, although at first useful and pleasant, became after a time intolerable, and produced a state of ennui upon the mind which appeared to keep up the malady. In this state, weak and emaciated as I was, I determined once more to resume my professional avocations, and as I had found by experience that a cold, damp atmosphere with a clayey subsoil was injurious to me, I chose this place for a residence, where I have now resided eight years; my health and strength gradually improving. The means which I have found most useful in my case have been a simple but nourishing diet, taken only in such quantities as the stomach would bear without a feeling of oppression, moderate exercise not amounting to fatigue, and agreeable occupation of the mind. I do not now suffer often from the disease, and when I do suffer, the attacks are trifling compared to what they formerly were. My firm belief is that I should not suffer at all if my mind was perfectly quiet and pleasantly occupied; but I have now a large practice, often much bodily and mental fatigue, and sometimes considerable exposure to the weather in long journeys; and as these are inseparable from the practice of a profession which I love with all my heart, I make up my mind to suffer a little rather than forego it. I have reason to think that the situation of Hastings is peculiarly favourable to my disease. I have never fully recovered the perfect use of my lower extremities, yet they are much stronger than formerly; for I used to require a stick to walk with, now I never or rarely use one. I do not, however, walk much, as I find I am soon tired."

Another singular affection I have lately witnessed in connexion with gout, I may mention here. A lady of a decidedly gouty habit, aged 60, applied to me under the following circumstances: for the last two months she had become liable to a daily paroxysm, which observed the following course. About three o'clock in the afternoon her nose began to grow hot, and the heat continued for four or five hours, the part becoming first of a bright, and then of a purplish red colour, which spread to the upper portion of her cheeks, and was accompanied by some uneasiness, but no pain, and always subsided about the same hour in the evening. I advised small doses of colchicum to be taken in this case.

In general, a regular attack of gout in the extremities is preceded by a longer or shorter period of constitutional disturbance and dyspepsia. We must not, however, in making the diagnosis between gout and rheumatism, consider this distinction as not liable to exceptions, for I have seen more than one case of hereditary gout, in which the arthritic attacks came on suddenly without the slightest precursory derangement of the health, or the operation of any assignable cause. I have as yet seen no instance of a similar nature in acquired gout.

Another exception to the general rule is also worthy of notice. In general, a fit of the gout is preceded and accompanied by a scanty secretion of turbid high-coloured urine. As the fit goes off, the urine increases in quantity, becomes clearer and paler, and loses its tendency to deposit the lithates and purpurates. Now, in two cases of hereditary gout, I have seen this order reversed, and the approach of the fit announced by a great increase in the secretion of urine, which was quite watery and limpid, and continued so until the violence of the articular inflammation began to decline. The urine then became scanty, and deposited the lateritious and pink sediment in great abundance.

That the gouty diathesis may excite its specific inflammation in most of the tissues of our organs is a fact generally admitted; but I regret to state that our knowledge concerning the effects which it produces in these various tissues is far from being accurate or extensive. Beere, M'Kenzie, Middlemore and others, have done much towards elucidating its effects on the eye and its appendages; and we are tolerably well acquainted with its progress in serous, synovial, and fibrous membranes. What changes it produces in the secretions of mucous membranes is a question which has not been studied with an attention commensurate to its importance. Thus, though all acknowledge the existence of gouty cough or bronchitis, the diagnosis and history of this affection are still very incomplete. This has been acknowledged by Dr. Stokes, who has published by far the best account of bronchitis which has yet appeared.* The effects of gout on the lining membrane of the urethra and bladder are better known and studied, but I think that much still remains to be done in this as in every other class of inflammatory diseases, where the inflammation depends upon a constitutional taint.

In my published lectures I have long since expressed an opinion at variance with that generally taught concerning the bronchitis and pneumonia which accompany pulmonary consumption, and I have brought forward strong reasons for believing that too much importance has been attached, and attention too exclusively devoted, to the tubercles in this disease. Thus, authors talk of tubercular pneumonia, where it would be more correct to designate the affection as scrofulous pneumonia accompanied by tubercles; they speak of tubercular cavities and abscesses in the lung, in cases where scrofulous cavities and abscesses exist. In fact, I repeat it emphatically, that the essential characteristics of phthisis pulmonalis are derived from scrofula. This it is which converts what would be common into consumptive pneumonia or bronchitis—this it is which so often renders both incurable.

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* "On the Diagnosis and Treatment of Diseases of the Chest," by W. Stokes, M.D. This work places its author among the first medical observers of the day, and has acquired for him a European fame.

Tubercles and tubercular infiltration are mere results of nutrition morbidly modified by scrofula; they are effects, not causes. They often exist without scrofulous inflammation, and the latter may exist without them. It gives me much pleasure to find that these opinions, which I published many years ago, have received ample confirmation from the observations of Dr. Kingston, in a paper read before the Royal Medico-Chirurgical Society of London, and shortly noticed in the *Medical Gazette*, April 29, 1837.

In pursuing the subject of my lecture, I shall now turn to the consideration of some phenomena connected with the gouty diathesis, which possess a much deeper interest, and lead to views of far greater importance. I mentioned before that we frequently observe flying pains, or twitches in various parts of the body, arising from a rheumatic or gouty cause; that in some instances these affections appear to be limited chiefly to the nervous trunks or branches, and that we have thus what may be termed gouty or rheumatic neuralgia. We are familiar with rheumatic or gouty sciatica, and we know that the history and termination of this form of disease often prove it to be inflammation of a specific character, chiefly confined to the trunk of the sciatic nerve. Now, it is not unreasonable to suppose that this specific inflammation of a nervous trunk or branch may, like other inflammations, extend farther, so as to involve parts of more importance to the economy.

What I wish to draw your attention to is this—that in certain cases, where gout attacks the nerves, giving rise to gouty congestion or inflammation frequently recurring, and acquiring increased strength and deeper root as it proceeds, the morbid affection may, after years, or even months, run on until it reach the spinal cord, involving a certain portion or portions of that organ, and producing loss of sensation and motion commensurate to the amount of spinal derangement. This is by no means an anomalous occurrence; it is merely an instance of disease originating in the periphery of the nervous system, passing along the trunk of the affected nerve with a retrograde motion, and finally reaching the central parts.

It is too much the custom to look upon paralysis as depending upon original disease of the nervous centres. When I come to speak of paralysis, I expect to be able to prove to you that, very often, disease commencing in the nerves of some particular part or organ may be gradually propagated to the spine, producing all the symptoms which are referable to an original affection of the nervous centres. I shall also bring forward numerous facts in proof of the propagation of disease from the circumference to the centre of the nervous system; and the pathological deductions to be drawn from these facts seem to me to include all the physiological discoveries made by Müller and Marshall Hall, concerning what the latter terms the reflex function of the spinal marrow. I shall be able to show you that enteritis, arising suddenly in two young and healthy persons, from indigestion and obstruction caused by an error in diet, was followed in both by well-marked paraplegia. I shall likewise bring before you examples of paraplegia connected with stricture of the urethra, and which were relieved by curing the stricture; and I shall detail cases of acute and chronic affections of the uterus and kidneys, which had entailed on the patients, as a remote consequence of the original disease, loss of the power of motion in the lower extremities, sometimes partial and curable, sometimes irremediable and complete.

The cases I am about to relate to you now are most interesting and valuable, and enable me to carry this principle still farther, by proving *that gouty inflammation of the nerves and their neurilemma may, in process of time, extend to the spinal marrow and its investments, and give rise to derangements of the latter, terminating in ramollissement and structural degeneration.*

The subject of gouty degeneration of the spinal cord has not been alluded to distinctly by any author with whom I am acquainted, and is, as far as I can learn, quite new. The deductions, therefore, which are drawn from my cases must, of course, be subject to such modifications as may be derived from future experience, and must remain to be confirmed by further observation. It has been long known that gout may attack the brain, and the existence of gouty paraplegia is well known by practitioners who have studied attentively the progress of arthritic affections. Thus, in a case which I witnessed some time back, in consultation with Mr. Kirby, he prognosed the supervention of paraplegia at a time when the indications of its approach could not have been discovered by any observer of less experience and sagacity.

I have already stated that gouty affections of the brain have long been known, and I am not sure that some of the older authors may not have alluded to gouty affections of the spinal marrow; but as our knowledge of the peculiar state of the brain and spinal cord, termed ramollissement, is comparatively recent, and not dating with any degree of accuracy earlier than the works of Abercrombie, Rostan, and other modern authors, it is obvious that any observations made by the older writers concerning gouty affections of the nervous centres can have no distinct reference to this lesion. The connexion, therefore, of ramollissement of the spinal cord with gout may be considered now, for the first time, distinctly pointed out. As one of the cases which I am about to detail presented an example of the most extensive ramollissement of the spinal marrow on record, it would, on this account alone, be especially deserving of attention; but its interest is increased tenfold when placed in juxtaposition with the second case, so as to exhibit in a striking point of view the close resemblance observable in the march or progress of both, as well as the identity of the lesion discovered after death.

Mr. ——, residing in the Island of Anglesey, was very much addicted to field sports, and, while thus engaged, would occasionally remain for a whole day without food. He was also very fond of angling, and has been frequently known to wade up to his waist in water for many hours together, during very cold weather. His general health was good, and his habits were abstemious. In 1825, when about twenty-five years of age, he had fever, attended with inflammation of the joints, and said to be rheumatic: some pain and stiffness, and an evident enlargement of the knee-joints, remained after the other articular affections had disappeared; these symptoms, however, yielded in a few months to rest and appropriate treatment. His health also improved greatly, and he had no complaint of any kind whatever until the autumn of 1828, when he had a slight attack of ordinary cholera, after returning from a shooting excursion.

In the spring of 1832 he was attacked with pain in one foot, supposed to be of a gouty nature. This pain disappeared during a drive of fifteen miles in an open carriage, but a certain degree of tenderness remained, and was always felt, more or less, in the part originally affected. He had a similar attack of pain and tenderness in the same foot in the following autumn. At the time when this attack commenced he was twenty miles from home, and observed that during his journey the pain became diminished as before, and in a few days subsided altogether. In August, 1833, he had a similar, but much more severe attack: the pain was much more violent than before, and both feet were affected. This, however, did not prevent him from following field sports as usual. He went on horseback to the mountains to shoot grouse; and to this exercise, and drinking a bottle of wine, he attributed his speedy, or rather sudden recovery from the pain in his feet.

Hitherto we have seen a naturally strong constitution struggling successfully against exposure to cold, imprudent habits, and a most injudicious method of disturbing, or rather repelling, local inflammation depending on a gouty diathesis. It is not easy to explain how it happened that driving in an open carriage, or riding over the mountains, so effectually cut short the paroxysms of gout in the feet; but it is enough to know that the fits were suddenly and imprudently arrested, to be prepared for the consequences which ensued—viz., an irregular distribution of the gouty effort, and its determination to internal organs.

In September, 1833—that is, about a month after the sudden subsidence of the last attack—he was seized with violent colic, accompanied by obstinate constipation. The pain was very severe, but he suffered more from a general feeling of restlessness (a restlessness beyond belief, as he expressed it) than from actual pain. He was also greatly annoyed by singultus, and was jaundiced after recovering from the attack of colic. In January, 1834, he had another attack of colic, preceded by a fit, the precise nature of which I was unable to ascertain. As these abdominal attacks frequently recurred, I shall give a description of one of them, as communicated to me by Dr. Llewelyn Jones, jun., his attending physician.

"A dull, wearing, and fixed pain would attack the patient in the region of the colon: this pain was not increased by pressure, and was accompanied by nausea, occasionally by vomiting, and always by obstinate constipation. These symptoms were attended with a most distressing sensation of restlessness and anxiety. They lasted on one occasion for three days and nights before I could get the bowels opened, when they were immediately mitigated. The pulse was never quickened, and in general remained natural; but if the attack was prolonged, it became weak. There never was any fever, nor any well-marked indication of inflammation in the abdomen. These attacks were always preceded or followed by a gouty affection of the feet."

The attacks in the stomach and bowels recurred frequently, and always with the same symptoms, until August, 1835, when a visible tremor of the fingers became observable: during some preceding attacks he used to complain of weakness of the wrists and pains in the fingers, particularly the last joints. As the disease progressed, these pains became more intense and extensive, and the torture he felt in the hands and arms was beyond description. After August, 1835, he began to lose the use of his arms, the tremors increased, and he began to complain of stiffness about the neck, with great restlessness and anxiety. The abdominal attacks came on occasionally, but not so severely as before. The arms became gradually weaker, until the loss of muscular power was complete, and they were greatly emaciated; but Dr. Jones, who had the patient under his observation until August, 1836, could not detect any evident diminution either in the upper or lower extremities, and the intellectual faculties remained perfectly unimpaired.

In October, 1835, two months after the state of the upper extremities had indicated the approach of paralysis, the lower extremities became similarly engaged; they were affected with tremors and weakness, and in the following December the patient had an attack of violent pain, with swelling and increased heat in the ball of one foot, which was pronounced to be of a distinctly gouty character. After each attack of pain in the feet, as I have been informed by this gentleman's sister, the loss of power in all his limbs increased, and if he gained a little strength in the intervals between these attacks, a recurrence of the paroxysms always made him worse than before.

In February, 1836, I went to Anglesey to visit this gentleman, and saw him in consultation with Dr. Jones and Dr. Williams, of Denbigh. After a minute examination of the history and symptoms of the case, I declared it to be my opinion that a gouty inflammation had attacked the nerves of the extremities, and had finally extended to the spinal cord and its sheath. I said that at an earlier period of the disease I would have advised salivation by mercury, but as that was inadmissible under the existing circumstances, we should have recourse to other measures. I forgot to state that, from the commencement of the disease, the advice of Sir B. Brodie and other eminent practitioners in London had been obtained by letter.

It would be useless to detail the various general and local remedies fruitlessly employed in this gentleman's case. He went to Liverpool in August, 1836, for the benefit of further advice; but finding no relief, returned to Denbigh, where he died in the ensuing October. For some time before his death he was greatly emaciated, and quite paralytic in all his limbs, but retained his faculties to the last. His body was examined by Dr. Williams, whom I had met in consultation in the preceding February. This gentleman informed me, that the viscera of the thorax and abdomen were healthy and normal, that no derangement or lesion of the brain could be detected, but that the spinal cord, opposite to the last cervical and first dorsal vertebræ, was softened to the consistence of thick cream; the remainder of the cord was also softer than natural, but did not present anything peculiar in other respects.

In a letter which I have since received from Mr. Williams, to whose kindness I am much indebted, he expresses himself with regard to the nature of the patient's disease in a way which confirms the views I have taken. He observes:—"I once saw Mr. —— in an attack of the gout in the feet, about three years before his death. There was much pain, and a decided gouty blush. Exposure in fishing and shooting to a very imprudent degree, while under the influence of these gouty attacks, I have no doubt did much to render the disease irregular and erratic."

The fact that the tremors and loss of power commenced in the arms two months before indications of paralysis of the lower extremities appeared, is sufficient evidence to prove that the spinal marrow was not the point from which the diseased action proceeded originally; for had this been the case, an affection of this organ, sufficiently violent to give rise to paralysis of the upper extremities so gradual in its progress, and so well developed, must long before this period have occasioned paralysis of the legs also. There is a striking analogy between the progress of the tremors and paralytic symptoms in this case and in cases of painter's colic; and the analogy likewise holds good as to the violent spasmodic affection of the bowels, and the constipation observed in both. It is further worthy of notice, that in painter's colic the nervous affection is accompanied by pain and weakness of the extremities, and ultimately, although long after the commencement of the disease, by spinal tenderness—a fact which has been already noticed by Dr. Bright. Again, in painter's colic, as in the disease which I have just detailed, the affection of the spinal cord and the consequent paralysis are evidently subsequent to the disease of the peripheral portion of the nerves.

The next case which I shall now proceed to detail is one of equal interest and importance. A gentleman of robust frame, aged about fifty-five, and having an hereditary predisposition to gout, to which his father had been a martyr, and which had exhibited itself in one of his sons at the early age of thirteen, consulted me on the 7th of June, 1836. Being a man of extensive landed property, he resided chiefly in the country, and was in the habit of using much active employment and exercise, but indulged rather freely in the pleasures of the table. After suffering much annoyance from dyspeptic attacks and various premonitory symptoms, he had a regular paroxysm of gout in the spring of 1828; he had a similar one in 1830, and another in 1832, each occurring, as before, during the spring season, and remarkably severe. During the year 1832 he had several slight returns of the complaint, and in January, 1833, had an alarming attack of an enteritic character, accompanied by spasms of the stomach and acute pain of the extremities. In the autumn of 1834 he suffered greatly from a nephritic affection, and got relief after passing a considerable quantity of uric acid gravel.

In the spring of 1835 he had a fall from his horse, and for some time afterwards complained of pain in the small of the back and around the trunk. He recovered, however, and during the summer and autumn of that year remained pretty well; but in the last week of December caught cold, which was followed by severe cough, and pains in the chest and feet: the latter were then considered to be the effects of gout. From this period his health, though often apparently restored, was never firm: he became subject to sudden attacks of pain, particularly in the chest, which gave him much uneasiness. On the 3rd of June he consulted a physician in his neighbourhood, to whom he described his ailment as "a slight pain in the right side, which troubled him only a short time before he got up in the morning;" this he stated he had felt occasionally for two months before. A very careful examination was made over the situation of the liver, the place in which he said he felt pain; but no tenderness or swelling whatever was detected, nor was there any in the direction of the spinal cord. His pulse was at this time perfectly regular, his bowels natural, and no dyspeptic symptoms existed. He used, by the advice of this physician, tonic and laxative pills and a stimulant embrocation.

When he consulted me on the 7th of June, 1836, I found him labouring under what appeared to me to be pleurodynia of an intermittent and gouty character. During the day he was perfectly free from pain, but in the evening the pain commenced, and continued with violence until morning. It is unnecessary to detail here the various local and constitutional remedies which I employed in this gentleman's case, but without any favourable result. From the middle of June his symptoms became worse; during the first part of the night his pains very severe; towards morning he usually obtained relief by lying on his face, and carefully avoiding all motion. About the latter end of July, the pain, which had been almost constantly felt at the right side, moved to the left, imparting at one time the feeling as if a spear were passing through the diaphragm, and at another resembling the sensation as if these parts were squeezed in a vice. When he was in the horizontal position this pain was accompanied by a sense of weight; and at times the pain would shoot upwards to the clavicles, producing tenderness of the intercostal spaces. When the diaphragm was free from pain, it most commonly attacked the postero-inferior edges of the scapula, and the dorsal region in its vicinity.

In August he tried the use of a warm bath, and found temporary relief from the first he took; he remained too long in the second, which was heated to the temperature of 100, and nearly fainted. He used the warm bath six or eight times, but found no material benefit from it, and could not bear the pain produced by the jolting of his carriage in going thither. About this time there was a visible alteration in his gait and figure; the left shoulder was elevated, his whole frame attenuated, and his face pale; he had nearly lost all power of bending the spine, and walked with a peculiar stiffness of gait as if his arms were pinioned. On the morning of the 21st of August he stated that he had suffered great agony during the night, and on its abating, considerable tumefaction was observable under the right ribs. Dyspeptic symptoms now became urgent; his urine scanty and turbid; he became melancholy, and his mind was wholly occupied with sad presentiments. At my recommendation he came to town, in order to place himself under my more immediate observation, and to have the benefit of a consultation.

About the 30th of August he got, to his great joy, an attack of gout in both feet; while this lasted, which was for about six days, he had complete relief from the agonizing pains in the diaphragm and chest. The interval of tranquillity was, however, but of brief duration; the inflammatory affection of the feet suddenly subsided, and the pain attacked the diaphragm with increased intensity. His strength, which had been rapidly failing, now gave way, and he became quite paraplegic. About the 10th of September the abdomen became engaged, without any alleviation of the thoracic symptoms, and he began to complain of constipation, tympanitis, and abdominal tenderness. The mucous membrane of the bladder became next affected; he had retention of urine, with great irritation of the prostate gland, and it was necessary to draw off the water with the catheter several times in the day. This state continued from the 22nd of September to the 10th of November, when the sphincter of the bladder became paralysed, and the urine drained off as fast as it was secreted.

During all this time the urine continued to present the characteristic marks of the lithic acid diathesis in an extreme degree, and contrasted strongly with the secretion furnished by the inflamed mucous membrane of the bladder, which consisted of a greyish or whitish yellow, viscid, and somewhat puriform mucus, containing either a free alkali or an alkaline carbonate. This secretion was extremely adhesive, and hung down in long ropy filaments when the vessel in which it stood was inverted. The nature of this mucus was such as to prevent any reaction from taking place between its own alkali and the acid of the urine. The co-existence of two secretions in the bladder, the one alkaline and the other acid, as observed in this case, is extremely curious.

In this way the patient's sufferings went on every day increasing, and requiring the most extraordinary care to produce any alleviation, a task which was discharged with the most indefatigable humanity and attention by Mr. Richardson, to whom I am indebted for most of the details connected with the earlier history of this case. About ten days before his death the extremities, upper as well as lower, and the trunk became quite paralytic; and from the cervical vertebræ downwards all power of motion and sensation was lost. His voice now became weak and inarticulate, deglutition was greatly impeded, and he finally sank on the 27th of November, 1836.

It may be necessary to state that at the time the paraplegia was beginning to seize on the extremities, the patient was much annoyed by occasional involuntary jerkings of the weakened limbs. This morbid action of the voluntary muscles continued when all power of voluntary motion had completely ceased.

This gentleman's body was examined twenty hours after death by Mr. Adams. The body and limbs were greatly emaciated, and there were several sloughing sores on various parts of the body and limbs, particularly over the scapulæ, sacrum, and ilium. The brain was perfectly healthy, with the exception of a slight effusion under the arachnoid, and into the fourth ventricle. On opening the spinal canal, which was done with extraordinary care and accuracy, the spinal marrow, from the fourth cervical vertebra down to its dorsal termination, was found converted into a morbid mass, of an ash-grey colour and pulpy consistence. The theca was quite healthy; but on the first transverse section

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mence in the spinal marrow or theca, but in the bones, and that the nerves, after their exit from the spinal cord, are affected in all cases before the cord itself. The reason is obvious; the affection of the nerves is secondary, and solely derived from their proximity to the inflamed bone and investing tissues; and consequently the nerves on one side may be affected, while the corresponding nerves on the other side escape for the time, and until the disease in the bone extends itself to their neighbourhood also. This view of the subject has not escaped the notice of German pathologists.

In the case above related, the pains continued in one side for months, and were then suddenly transferred to the other, an occurrence which is quite irreconcilable with the idea of their dependence on primary spinal disease. The well-marked ease the patient experienced when the gout appeared in the feet, and the perfect intermissions of pain which he frequently enjoyed during the earlier stages of the complaint, afford strong evidence that the pains, however violent and excruciating they might have been during the paroxysms, did not depend on an original affection of the spinal cord. Had the fall which this gentleman received, or any other injury, induced inflammation of the spinal cord and subsequent degeneration of structure, the order and course of his symptoms would have been very different, and long intervals of comparative ease would not have intervened between the appearance of the first pains and the subsequent paralysis.

When paraplegia originates in disease of the spinal cord itself, retention of urine, or irritability of the bladder, often announce the approach of the disease long before the loss of power in the limbs becomes evident; whereas, in all those cases in which the paralysis creeps from the extremities along the nerves towards the spinal marrow, the bladder is affected only at a late period of the disease, as occurred in the case which I have just detailed. Finally, the remarkable similarity which exists in various points between this case and that of the Welsh gentleman, who had never met with any accident or injury, and in whom a considerable degree of ramollissement was observed, leaves no doubt that in both instances the disease commenced with gouty neuralgia and inflammation of the nervous extremities and their sheaths, which gradually extended to the central portions of the nervous system, and ultimately involved the spinal cord.

It is of great importance that you should be aware of this termination, and know that in gouty habits the sad results already noticed may be produced: particularly as a knowledge of this fact may lead to the timely adoption of preventive measures. Having experienced the total inefficacy of colchicum, hydriodate of potash, strychnia, and all the usual remedies, in relieving or removing this form of disease, I would be strongly inclined to recommend the early insertion of issues over the spine, with prompt and decided mercurialization. The late Mr. Colles has recommended the use of mercury in paraplegia, and cites some cases in support of the utility of the practice. It is to be regretted that he has not given any hints as to the mode of diagnosing the cases likely to be benefited by the mercurial treatment, from those in which mercury would be inadmissible. Hence his recommendation loses much of its value, and cannot serve as a guide to those who have to treat spinal disease connected with paralytic symptoms. It appears, however, sufficiently plain that mercury, employed at an early period of the disease, is most likely to prove serviceable where symptoms of paralysis arise from inflammatory affections of the nerves or their neurilemma, or of the spinal cord and its sheath.

So far at present on the subject of paralysis as connected with the gouty diathesis. I hope to be able in a short time to bring it again before you in a more complete and extended form.

In the preceding observations we proved that gout often attacks the nerves of the extremities in the first instance, and then pursues a retrograde course until it reaches the spinal marrow. It is an acknowledged character of gout that it wanders from one organ to another, and that it is very uncertain as to the periods and duration of its attacks, sometimes appearing to have ceased altogether, again only to return with redoubled violence. These characters of gout are strikingly displayed in the two cases I have related, where it finally seized on the spinal marrow; and it is quite possible that what took place towards the fatal terminations of these cases may in other gouty subjects occur at a much earlier period, and without the previous occupation by the disease of the nerves of the extremities; indeed, there is no reason why gout should not attack the spinal marrow and its investing membranes in the first instance, or in consequence of metastasis. That rheumatism, the disease most closely allied to gout, may do so has been proved by numerous examples, of which we owe some of the most striking to Dr. Copland and Dr. Prichard, for the result of whose researches on this subject I must refer you to the article Chorea in Copland's *Dictionary of Practical Medicine*, where you will find that rheumatism not unfrequently produces both acute and chronic inflammation of the spinal membranes. These observations I make with the intention of proving that my views concerning gouty affections of the spinal cord are borne out by analogy, and the experience of others with respect to rheumatism.

LECTURE XXXII.

RHEUMATISM.—SCIATICA AND LUMBAGO.

I SHALL commence to-day's lecture, gentlemen, with a few observations on that rheumatic affection of the joints, to which I shall give the name of *arthritic rheumatism*. You will meet, in practice, with cases of arthritic rheumatism attended with fever, where, after the violent inflammatory symptoms have subsided, the arthritic inflammation will continue to wander from joint to joint, sometimes almost entirely vanishing, and then again reappearing. You entertain hopes of getting your patient over the disease, and he is indeed better; but, on your next visit, you find that the pain has fixed itself—suppose in the wrist joint. If such a pain as this should appear while the acute symptoms are present, besides the general remedies you will employ local means of relief; and some persons, as for instance, Dr. Elliotson, would make cold applications to the part: but this I do not approve of, nor would I recommend you to practise it. However, generally speaking, your treatment consists in leeching the affected part, the internal exhibition of colchicum, &c.

What I would urge upon your attention is, can you trust to leeches on all occasions, and at every period of the disease? No; there is a period when you must blister; there is a time when stimulant and tonic applications become indispensable. The general treatment of every case of arthritis must close with tonics. First, you pursue the antiphlogistic treatment, next you employ specific remedies, and lastly, you have recourse to tonics; and so likewise with the local applications. In the beginning, local pain, tenderness, and swelling depend on active inflammation, and yield most readily to leeching. As the disease advances, the number of leeches which each fresh appearance of local inflammation requires is comparatively less, and finally, the local affection, on its recurrence in any joint, is of such a nature, that leeching is no longer proper, while certain and almost immediate relief may be obtained from blistering. Blisters are better than leeches, not only because they possess the power of removing pain and swelling with more rapidity, but also because they do not leave the part in a weakened state. I tell you that blisters have a powerful effect in removing such pains, and that they may be used in cases of arthritis where they have not been used heretofore.

There is another practical observation on rheumatism which I made before in the hospital wards. Cases of arthritic rheumatism will come under your notice, in which the pain and fever are, from the beginning, accompanied by sweating, and this sweating is not attended with any relief; the pulse remains quick, the fever persistent, and the pain undiminished. This sweating, be assured, never tends either to diminish fever or relieve pain; and this is the kind of arthritis which is most apt to terminate in confirmed affections of the joints, and may last for life. From my experience elsewhere, and from the observations I have made in the Hospital for Incurables, I have remarked that most of those persons whose limbs are permanently stiff, or even distorted from rheumatic affections, have been suffering for years under this sweating arthritis. In one of the patients at that institution a curious effect followed this disease. The sweating was general over his body at first, but after some time it declined in the lower extremities, which seemed incapable of sweating any longer. The cuticle over these parts began to exfoliate and become dry and rigid. A still further change took place, and the lower extremities became covered with ichthyosis. He lies in bed on his back in a helpless state, his legs and thighs covered with a horny unyielding cuticle, but his breast and face continue to sweat profusely as before.

Bear in mind, therefore, that this form is liable to terminate, as I mentioned before, in incurable arthritis. Some of the senior students may, perhaps, recollect a poor man in the chronic ward of this hospital, who laboured for month after month under this torturing malady. He lay in a corner in this state, and it was a subject of constant regret to everybody to see him in this pitiable condition without any prospect of relief. Practitioners are apt to make a mistake in the treatment of this disease. They find the pulse quick but very seldom strong, and rather forbidding than indicating the abstraction of blood. How are you to treat such cases? By the use of the lancet. Begin, however, cautiously; take away, at first, about five or six ounces of blood, and observe what effect this produces. If your patient's pulse is improved, his pain lessened, and the sweating diminished, you are encouraged then to bleed more boldly. Venesection is here our sheet anchor. You have seen how much relief it gave the man above stairs, and what a remarkably buffy coat his blood presented. His sweating was diminished considerably by this means; and did you remark how I got rid of this symptom entirely? By giving him minute doses of tartar emetic and opium. He had a mixture composed of half an ounce of the solution of tartarized antimony, and half a drachm of tincture of opium in sixteen ounces of water; of this he took half an ounce every hour. It is hard to account for this, but it is a fact, that in some cases of chronic sweats, particularly those which attend hectic fever, you can put a stop to them by giving a few grains of Dover's powder at bed-time.

There is another very remarkable case at present in the house which bears upon the observations I have been now making; I allude to the patient with sweating arthritis, to whom I drew your attention this morning. This poor man, who is somewhat advanced in life, has been labouring for several months under inflammation of the joints of a rheumatic character, manifesting itself by pain, stiffness, swelling, and probably some slight effusion into the synovial membranes. These symptoms were accompanied by profuse and constant perspirations, with a tendency to diarrhœa—circumstances which caused a manifest deterioration of his health and strength; he became pale, cachectic, and emaciated. His case has been very tedious and intractable; he had been a long time in the hospital, and had used all the most appropriate remedies but without any appreciable improvement; his joints remained stiff, painful, and almost useless; he was greatly reduced in strength, and entirely confined to his bed. In addition to this, his pulse continued unreduced in frequency, and this is always a bad sign; cases of rheumatic arthritis, attended with prolonged excitement of the circulation and copious sweating, are generally found to exhibit an intractable chronicity, and too often terminate in rendering the unfortunate patient a cripple for life.

Now in this case many remedies had been tried without effect, and the state of the man's constitution, combined with the circumstance of his having a tendency to bowel complaint, contributed to reduce still further the scanty list of our remedial agents. Alterative remedies, to affect the general system, were almost entirely out of the question, and a vast number of local applications had proved unsuccessful. It occurred to me here, that some benefit might be derived from mercurial ointment, gently rubbed over the affected parts, assisting its action by the use of rollers applied round the joints. Fortunately the experiment proved successful; in the course of a week or ten days the swelling diminished considerably, the pain is nearly gone, and the power of motion is returning. His mouth has become affected, but the relief experienced appears to be proportioned not to the influence of mercury on the general system, but to its effect on each individual joint. As a proof of this, I may state that the man has been mercurialized before, but without any favourable result.

Here, gentlemen, is an important point for consideration. A patient labours under a certain number of local inflammations, for which mercury is given internally, so as to affect the mouth, but without any manifest improvement of symptoms; we afterwards try the same remedy in another form; we apply it locally, in the shape of ointment rubbed into the skin over the diseased parts, and we succeed in giving relief. This is a fact deserving of attention. You will perhaps ask me to explain this; I cannot do it; but I can bring forward many other analogous examples. If you refer to the late Mr. M'Dowel's valuable paper on Erysipelas, published in an early number of the *Dublin Medical Journal*, you will find that many cases of this affection derived great benefit from the use of mercurial ointment; in fact, much more than they could by giving mercury internally.

In the next place, I have met with many cases of enteritis and peritonitis where the disease continued after the system became affected by mercury; and I have observed that these cases yielded rapidly to blistering the abdomen, and dressing the raw surfaces with mercurial ointment. Sir H. Marsh and I attended a young gentleman lately, who had low fever, accompanied by a quick but feeble pulse, and great restlessness. About the tenth day his belly became tender and exquisitely painful; he had thirst, diarrhœa, and other symptoms of enteric and peritoneal inflammation. Before his illness he had been of rather delicate habit, and had further impaired his health by close study. He was therefore unfit for depletion, and of this we were convinced by the debility which followed the application of a few leeches. Under these circumstances we ordered a large blister to be applied to the abdomen, and the vesicated surface to be dressed with mercurial ointment. This proved eminently successful; the peritonitis, enteric irritation, and fever soon disappeared, and the young gentleman recovered completely.

The same thing is seen in many cases of pleuritis; the constitutional effect of mercury will fail in removing the affection of the pleura until it is applied locally. I might also refer to instances of common inflammation of the testicle, in which mercurial ointment smeared over the part has been found decidedly beneficial. It is unnecessary for me, however, to multiply examples; what I have stated gives ample proof of the utility of mercury applied locally. When I was a student, it was the fashion to scout the doctrine that any distinct effect could be produced by the local application of mercury; our teachers laid it down as an axiom, that, to produce any sensible effect, it was necessary that it should first enter the system through the lymphatics. Thus, when you rub mercurial ointment over the liver to remove hepatic derangement, they said, before it could exert any influence on the liver, it had to pass along the thoracic duct, become mixed with the circulation, and manifest its peculiar action on the whole economy. Hence, in a case of hepatitis or testitis, it was deemed useless to apply mercurial ointment over the liver or testicle, since it had, as they expressed it, to go its round through the whole system, before it could affect either of these organs.

This reasoning has an appearance of plausibility, but it is contradicted by facts. Numerous examples might be cited to prove that the greatest advantage may be derived from the local application of mercury, independent of any effect produced by it on the general system. How often do we see an incipient bubo dispersed by mercurial frictions, before any constitutional effects occur? How frequently do we see laryngeal and hepatic inflammation relieved by the use of mercurial ointment without salivation? Do the beneficial effects, which we so often observe from the emplastrum ammoniaci cum hydrargyro, depend necessarily upon the mouth being affected? Is the relief which follows the use of mercurial ointment in erysipelas or testitis unattainable, unless preceded by mercurial action in the whole system? Indeed, any person who reviews this subject dispassionately, must see that the doctrine of a preliminary constitutional affection being absolutely necessary, in order to obtain the specific action of mercury on any particular organ, is wholly untenable; while, on the other hand, there is a host of evidence to prove that, locally applied, it produces a primary and distinct effect, totally independent of its action on the general economy.

Having spoken now of the utility of mercury in certain cases of rheumatic fever, where the inflammation of the joints will not yield to other means, I have to add that the hydriodate of potash has been found to be a most useful adjunct to mercury, and well calculated for following up and completing the beneficial effects produced by that remedy. In fact, in treating arthritic or rheumatic fever, when I have reduced the violence of the fever and of the inflammatory affection of the joints by means of bleeding and leeching, followed by tartar emetic or nitre, or both combined, or when, after the antiphlogistic treatment both local and general, I have produced marked alleviation of the patient's sufferings, either by the use of colchicum or by the use of mercury combined with opiates—then, I say, we can employ the hydriodate of potash with the greatest possible advantage, as it quickly dissipates the remaining pain and swelling of the joints, and contributes powerfully to bring the disease to a speedy termination, while at the same time it greatly diminishes the danger of a relapse. I have experienced much comfort and feel much confidence in the treatment of rheumatic fever since I adopted this practice; and it now never happens to me to meet with cases which, in spite of all my efforts, become chronic, and confine the unfortunate sufferers to bed for months. You have observed recently that, in most cases of acute rheumatism affecting the joints, no matter what mode of treatment I adopt in the commencement and during the acme of the disease, I generally complete the cure with the hydriodate of potash, beginning with doses of ten grains, which are quickly augmented to twenty or thirty grains three times a day. It is generally given in decoction of sarsaparilla, to which some preparation of morphia forms a useful addition.

Having said so much, I shall make but a few observations on another case of rheumatic fever we had lately in hospital. The patient had at first fever and inflammation of the joints; the fever was removed by appropriate treatment, but the inflammation of the joints continued; the fever set in again, and the arthritic affection increased, and we removed both. He relapsed again, the fever re-appeared, but there was no inflammation of the joints. Here we have a man admitted with rheumatic fever and inflammation of the joints; we try to cure the disease, and we succeed in removing the fever, but the joints remain inflamed; we remove this also, and congratulate ourselves on a recovery, and we again have fever and arthritic inflammation; we overcome this; and again a relapse comes on: but mark the difference; we have now fever, but the joints are unaffected. This is a curious circumstance, and confirms me in an opinion I have entertained for some time, that we may have rheumatic fever without inflammation of the joints.

Rheumatic fever is usually distinguished by being accompanied with pain, swelling, and redness of the joints; but I have remarked, long since, that this fever presents several other peculiarities. We have, as in other fevers, great heat, occasional tendency to sweating, and hard quickened pulse; we have the urine at first pale, then high-coloured, and the blood buffed. But we have no affection of the sensorial functions, no headache, and, when pain permits, rest; sleep is not proportionally impaired, the tongue is furred, but the appetite is frequently good, there is no nausea, no disgust at food. These peculiarities I have frequently remarked, but it was only lately that I became aware that this species of fever may exist without inflammation of the joints. It is well known, that the affection of the joints may exist without the fever. The combination of these two distinct, but frequently associated, affections, constitutes the disease termed rheumatic fever. This explains the reason why we must wait until a certain period, until the fever subsides, before we give tonics. We commence with antiphlogistics, then we employ specifics, such as mercury or colchicum, and afterwards we give tonics.

The case of Coghlan, who has been for some time an inmate of our chronic ward, demands a few observations. He was admitted for an attack of arthritis on the 10th of December, and since that period has been subjected to various modes of treatment. You will recollect that on his admission he stated that he had been attacked several times with rheumatic inflammation of the joints. Like most persons of his class, he has suffered greatly from repeated fits of illness, brought on by exposure to the same causes. One of the greatest misfortunes that can fall upon labouring men is a severe attack of rheumatic fever, accompanied by inflammatory affections of the joints; it not only renders them helpless and useless for a considerable time, but also in some cases leaves them cripples for life, and, in addition, the nature of their employment constantly exposes them to relapses, which at length bring on incurable affections of the joints; we have, moreover, in this young man's case, a combination not unfrequent in patients of this description, namely, the effects of cold on the chest as well as on the joints—arthritis combined with inflammation of the bronchial mucous membrane.

Now where the arthritic affection is very severe, and accompanied by high fever, the addition of bronchitis is a great aggravation. Every time the patient coughs he feels like one stretched upon the rack; at every convulsive motion of the chest a severe pang is felt in every joint, and the ordinary rate of suffering is increased to positive agony. A case of this kind is often hard to be managed, even when the disease is recent and the constitution sound; but when you have to treat a severe attack in a person who has repeatedly laboured under the disease, and whose vigour has been consequently impaired, the difficulty is greatly increased. Here much attention is required on the part of the physician. Where the combination is met with in a primary attack, I am generally disposed to regard both affections as of the same character, and not requiring any difference of treatment; I therefore attack the arthritis and the bronchitis with the same remedies, that is to say, venesection, leeches to the affected joints and over the chest, and large doses of nitre and tartar emetic. These remedies, however, are only calculated for the acute stage of a primary attack, and where the patient's strength is unimpaired; for when the disease is chronic, and debility present, you cannot venture on the use of large doses of tartar emetic and nitre.

In such cases much benefit is derived from the use of colchicum, particularly where the patient labours under more or less fever. The following is the form which I am in the habit of using, and from which I have occasionally derived much benefit:—

R. Misturæ Amygdalarum, f℥viij.
Aceti Colchici, f℥ss.
Acetatis Morphiæ, gr. i.
Nitratis Potassæ, ʒss.; Fiat mistura, cujus sumat cochleare unum amplum omni vel secundâ quâque horâ.

In Coghlan's case we tried this mixture, with local applications to the joints and a blister to the chest, but found at the end of some days that there was no visible improvement in the patient. Now, whenever a state of things of this kind occurs, no time should be lost; for, rely on it, that where colchicum does not afford relief *in a short time*, and *in moderate doses*, there is no use in giving it a further trial. You have here to contend with two affections of a very serious character—one capable of rendering your patient a cripple for life, the other threatening him with suffocation, from an extension of the inflammation into the minute bronchial tubes—an occurrence which is most commonly followed by dangerous congestion of the lung. Under such circumstances, the only treatment you can adopt with a hope of speedy relief and ultimate success, is to lay aside all other remedies, and trust almost exclusively to the use of mercury. In cases of this kind do not hesitate a moment, but mercurialize your patient at once, if his constitution be at all capable of bearing it. The treatment which was followed in the case under consideration was this:—we gave the patient ten grains of hydrargyrum cum cretâ four times a day; and with the view of relieving pain and the irritation of the bronchial mucous membrane, he took one drop of hydrocyanic acid, and ten drops of tincture of hyoscyamus in half an ounce of almond emulsion, three times daily.

The next affection I shall draw your attention to is chronic rheumatism, of which we have a well-marked instance in the man who lies in the chronic ward immediately under the window. He complains of pain, weakness, and numbness of the lower extremities, for which he used the decoction of sarsaparilla and small doses of corrosive sublimate, for a fortnight, without any obvious improvement in his symptoms. His complaint is of considerable duration, it being now fifteen weeks since he was first attacked. This, I need not tell you, is a very unpromising feature in his case. When rheumatism has continued for three or four months, it becomes a very intractable disease; indeed, there is scarcely any affection which tasks the ingenuity and tries the patience of a medical man more than chronic rheumatism. In this case, however, we have been so fortunate as to hit on a remedy suited to the complaint; the man has been rapidly improving within the last fortnight, and is now nearly well.

You will recollect that, when I undertook the treatment of this case, the patient was free from fever, his general health but little impaired, his pulse tranquil, his appetite good, no remarkable tenderness or redness of the joints—in fact, nothing to indicate the existence of acute local inflammation; consequently, it would have been useless to have recourse to leeches or blood-letting, or to administer antimonials, nitre, or colchicum. In such cases as this, a different line of practice must be followed; you must have recourse to stimulant diaphoretics—remedies which will increase the secretion from the skin, at the same time that they exercise a stimulating action on the nervous and capillary systems. Accordingly, we prescribed for this man the following electuary, of which he was to take a teaspoonful three times a day:—Powdered bark ʒj, powdered guaiacum ʒj, cream of tartar ℥j, flowers of sulphur ʒss, powdered ginger ʒj, to be made into an electuary with the common syrup used in hospitals.

The guaiacum not only acts on the nerves, tending to remove chronic pains, but also acts on the skin; you will find these, and other properties possessed by it, detailed at large in your works on Materia Medica. Whether given in the form of powder or tincture, it often proves an extremely useful remedy in cases of chronic rheumatism, where no symptoms of active local inflammation or general fever exist; where either of these are present, it is inadmissible. Ginger has also a stimulant effect, although its action is much more limited. It is a favourite domestic remedy, and is very frequently prescribed by our rival candidates for therapeutic celebrity—old ladies—in cases of chronic, or, as they term it, cold rheumatism; and I must confess that I have seen letting, general and local, followed by the exhibition of antimonials and Dover's powder in proper doses. Cupping the lumbar region (when the operation is skilfully performed) deservedly enjoys a high reputation in lumbago ; and if sciatica is present, the tender parts about the buttock and thigh must likewise be repeatedly cupped. In the latter case, it is requisite to have a very small scarificator, and cups of a corresponding size, so as to enable the operator to follow the track of the sciatic nerve. In Germany they generally use instruments so small, that ten or twelve of the glasses may be placed close to each other, in a line extending along the painful portion of the nerve, where it lies most superficially in the thigh. This practice deserves more general imitation in this country.

That popular remedy, a warm bath, often forms a most excellent adjuvant to these measures, and is still more efficacious when preceded by a powerful warm douche. A stream of hot water, played with considerable force against the loins, buttock, and thigh, seems to act not merely by the heat it imparts, but also by the mechanical impulse it exerts, an impulse which may be termed *water-champooing ;* in Dublin such a douche and warm bath may be had at the Northumberland Buildings. The means just enumerated, combined with absolute rest in bed, will succeed in many cases ; in others they will fail, and then this question arises,—What ought we to try next ? In some cases I have followed the example of Dr Percival and Dr. Cheyne, of giving two or three grains of opium in the day, combined with calomel and James's powder, and with much benefit.

In a case of lumbago and sciatica, which I treated with Mr. White, the disease at first neglected had passed from a chronic to an acute state, and had become painful to the greatest possible degree ; in truth, the patient's agony was quite excruciating, and though a man of strong mind and vigorous nerves, the sweat poured down his face from the suffering he endured whenever it was necessary to move himself in bed, or even when the floor of the room was shaken by any person treading heavily.

As our patient had been repeatedly cupped, and the usual remedies had completely failed, Mr. White proposed a combination of three grains of acetate of morphia, six grains of calomel, and twelve of James's powder, divided into eight portions, one to be taken every third hour; the good effects of this combination were so striking, that I have since had recourse to it repeatedly, and there is no one remedy in which I have greater confidence. Still, however, it is liable to the objection, that it must generally be continued until the gums become tender, or even the mouth slightly sore, an objection not, it is true, of much weight in cases like the preceding, where the disease is very acute, and the patient necessarily confined to his bed; but which renders this combination quite inapplicable in subacute or chronic attacks, where the sufferer tries to pursue his ordinary avocations, and is necessarily exposed, more or less, to the open air; to such persons *hydriodate of potash* will prove most valuable.

I first became acquainted with the remarkable efficacy of this medicine in lumbago and sciatica under the following circumstances. In the memorably wet month of July, 1839, I was called out of bed at midnight, to visit a lady in the country, and the vehicle sent to convey me was a hack covered car. The cushions were very damp, and I had not proceeded half a mile before I was attacked with lumbago so severe that I could scarcely walk when I arrived at my patient's residence. Next morning I was better, having perspired much during the night; but still the pain was troublesome, and as the season continued unusually cold and wet (indeed, it scarcely ever stopped raining from the 8th of July, 1839, to the 19th of February, 1840), and as my duties exposed me much to the weather, and prevented me from giving myself the necessary rest, my lumbago continued to increase again, and in about a month the gluteal and sciatic nerves of the left side became engaged; I noted particularly, that the pain spread very gradually downwards from the lumbar region, so that it took a week or ten days to arrive at the ham, and still a longer time at the ankle; I was then quite lame of the left leg, suffered much pain in bed, and had become so helpless, that I had to get my servant to draw on my stockings; during all this time my general health was perfect; appetite good; digestion regular; and no deviation of the urine from the natural appearance. I mention this, because several of my medical friends advised me to take antibilious aperients, an advice founded on Abernethy's doctrine, that many local affections proceed from stomach derangement.

I was at last forced to try something for my relief, and had myself cupped, and tried the warm douche and Dover's powder, but without any good effects. I began now to fear that I should be forced to give up all professional business, and confine myself to the house for many weeks in order to go through a mercurial course, combined with proper topical applications, when, happening to meet the late Mr. Ferguson, of Kildare Street, he recommended me to try hydriodate of potash, of which he was good enough to send me a drachm dissolved in a pint of decoction of sarsaparilla. I took quarter of this daily, and may literally apply here the common phrase, that I felt each dose do me good; in truth, the benefit I derived was perceptible hourly, and was so rapid, that in four days all traces of the lumbago were gone, and my lameness had quite ceased. I did not take more than one bottle—one drachm of the hydriodate, but the good effect continued after I had ceased taking it, and in less than a week I was perfectly well. Subsequent experience enables me to recommend this medicine strongly, in subacute and chronic lumbago and sciatica.

It is right to observe, that the remedy had in my own person to work against various disadvantages, for I neither relaxed from my labours, nor refrained from eating and drinking as usual. This is only another example of the many I have met, which prove how injudicious it often is to seek the cure of local inflammations by means of lowering the whole system.

In spite of the best directed means, sciatica is very apt to become chronic, and then oil of turpentine, carbonate of iron, arsenic, extract of stramonium, corrosive sublimate, blue pill and iodine internally, blisters to the loins, thigh, and calf of the leg, acupuncture, croton oil frictions, and other stimulating applications must be successively tried. On a former occasion I recommended a combination of opium, with oil of turpentine internally, and when that fails, Dover's powder, combined with sulphate of quina. I am sorry not to have it in my power to lay down any general principle which would enable you to judge in what cases each of these remedies is peculiarly indicated, for experience has not confirmed any of the rules generally relied on, and, therefore, we must content ourselves with treating these diseases empirically.

Change of climate, and the use of the Bath, Buxton, Harrogate, and Tunbridge Spa waters, have proved serviceable to many, while others have been obliged to have recourse to the natural hot baths of Bagnères or Barèges. In very obstinate cases, the practice long adopted in the surgical wards of the Meath Hospital, is to apply the actual cautery to five or six spots along the course of the painful nerve. The application ought to be rather severe, so as to produce moderately sized sores, which must be kept open for a fortnight or three weeks by suitable dressings. This is a very painful process, and for several days after the application of the cautery, the patient suffers much, and often thinks the disease to be aggravated; after some time, however, improvement becomes perceptible; and, on the whole, I do not think any other remedy is so much to be relied on in very obstinate cases of sciatica.

An observation made by Dr. Grogan is worth recording here, as it bears on a physiological question. In a patient of his, a young man of robust constitution, who suffered for more than a year much pain from an imperfectly cured sciatica, the affected thigh and calf were much subject to spasmodic pains and muscular twitchings. These sometimes continued night and day, and in consequence of these morbid contractions constantly recurring, the muscular fibres became hypertrophied, and the whole limb became much developed, presenting a more athletic outline, and exceeding its fellow considerably in bulk. This fact, which was pointed out to me by Dr. Grogan, is very remarkable; for, in general, chronic sciatica induces a flaccid and atrophied state of buttock, thigh, and calf. In the case referred to, the hypertrophy disappeared in less than a month after the actual cautery had been applied.

As the practical physician ought not to neglect any circumstance, however trivial it may appear, which bears upon the health of his patients, the following hints should not be regarded as too trifling for notice. Persons subject to lumbago ought, as much as possible, avoid remaining for any length of time in a flexed or stooping position, particularly if exposed to cold; it is for this reason that lumbago so frequently attacks gentlemen when engaged in the act of shaving. Those who are liable to the disease, therefore, must be careful either to shave while sitting before the glass, or, if standing, let the glass be placed so high that they may stand quite straight. Again, many are attacked while drawing on their boots; this accident may be surely avoided by using boot-hooks, with shanks about fourteen inches long, so that the body and thigh may be nearly in the same line, when the effort to draw on the boot is made. Persons who are in dread of lumbago and sciatica ought always to wear stout drawers, whose waistband should be broad, and consist of a strong, warm, yet elastic material, so as to allow it to be worn very tight without inconvenience.

DISEASES OF THE BRAIN AND NERVOUS SYSTEM.

LECTURE XXXIII.

PATHOLOGY OF NERVOUS DISEASES.

Before I proceed to speak of diseases of the brain and spinal cord, I wish to draw your attention generally to the pathology of nervous diseases. The subject is interesting, and one on which my opinions differ from some of those generally received. The observations I am about to make will involve the consideration of the general principles suited to guide us in the difficult study of nervous affections, rather than the description of any particular disease. In considering the symptoms that accompany diseases of the nerves, pathologists have directed their attention almost exclusively to the nervous centres, and have looked on the brain, cerebellum, and spinal cord as the parts in which the causes of all nervous disorders reside, or in which they originate. If you examine the works of Rostan, Lallemand, Abercrombie, and all those who have written on diseases of the nervous system, you will find that their inquiries consist in searching after the causes of functional changes, either in the cerebrum, cerebellum, or spinal marrow, forgetting that these causes may be also resident in the nervous cords themselves, or their extremities, which I shall call their *circumferential parts*.

When we recollect the manner in which the nervous system grows,—when we call to mind the fact that, in the development of that system during the fœtal state, the nervous extremities and trunks are formed before any traces of the brain are discernible, we must at once allow it is by no means improbable that these parts may become incapable of discharging their functions in consequence of changes originating in themselves, and not proceeding from the nervous centres. In a word, may not the decay and withering of the nervous tree commence occasionally in its extreme branches? and may not a blighting influence affect the latter, while the main trunk remains sound and unharmed?

In fact, gentlemen, pathologists have, with respect to diseases of the nervous system, continued an error precisely similar to that which was so long prevalent with regard to diseases of the vascular system; for it is only lately that, in estimating the forces which influence the circulation in diseased parts, they have begun to appreciate the preponderating influence of the capillary vessels, independently of the heart's action and the vis a tergo. It is only lately that they have recognized the important truth, that diseased vascular action may commence in the circumference.

I am willing to allow that in most cases of general paralysis the affection of the muscular system is produced by disease of the nervous centres; yet I think it is also evident, that an injury of the extremities or circumferential parts of the nerves may cause such a derangement of their functions as to give rise to paralysis. The reason why persons seek for the explanation of paralytic symptoms by referring them to the nervous centres, rather than their peripheral extremities, is because this mode of inference accounts more satisfactorily for the simultaneous affection of many parts of the system. Thus, if one hemisphere of the brain, or both, or if the cerebellum or spinal cord be pressed or injured, those parts which have a nervous connexion with them will experience a corresponding derangement of function. But if a process of disordered action be set up in one part of the nervous extremities, and this passes on to another part, the translation seems very strange, and you cannot easily comprehend why paralysis of one principal part will produce the same disease in another.

It has been asked, whether a local paralysis ever can, by spreading *towards the centre of the nervous system*, produce paralysis in another and a distant locality. This is a question we are not in the habit of investigating; and I think it has never been sufficiently or satisfactorily examined, considering its importance in a practical point of view, and the new light which it may throw on many of the most obscure and perplexing forms of disease. I shall endeavour to prove, first, that paralysis (from whatsoever cause it may arise) affecting one portion of the circumferential extremities of the nerves, may also affect other portions of their extremities; secondly, that pain originating in one situation may produce a similar sensation in distant parts; and, thirdly, that convulsions resulting from irritation in any part of the extremities of the nervous system may occasion a corresponding train of symptoms in other parts of the body. You perceive, gentlemen, that I have enumerated the three most remarkable symptoms resulting from the disease of the nervous system, namely, paralysis, pain, and convulsions. If I succeed in showing that each of these may be produced by causes acting on the extremities of the nervous system at a distance from the part affected, the position I have advanced will be proved.

A few days ago, happening to call at a gentleman's house, I was told by a young lady that she had wounded the inside of the ring finger with a blunt needle, and that she found in it a considerable degree of numbness and loss of sensation. I said to her, "Your little finger is also numb." You are aware these two fingers are supplied by the same branch of the ulnar nerve. Well, the little finger was really numb, as well as the finger next to it, which had been injured. What were the circumstances of the case in this instance? The side of the ring finger next to the little finger had been wounded with a blunt needle; the impression made on the nervous extremities of the side of one finger produced numbness not only in that finger, but also the same cause operated backwards, or towards the centre, so as to affect the branch given off to supply the little finger by the ulnar nerve, above the place of the wound. Here is an instance of a cause producing numbness of a particular branch of a nerve, occasioning the same affection in another branch, and giving rise to phenomena identical with those which might arise from an injury of the main branch of the ulnar nerve. This is a plain fact.

You have a case of precisely the same paralysis in a poor woman in this hospital, who has been complaining of rheumatic pains in various parts of her body. Before I had been struck by these and other instances of the same kind, I looked for the cause of this paralysis in the trunk; now I can understand how it may be in the periphery. You recollect I made some observations before on this subject, and mentioned that this numbness is frequently remarked in cases of gout and rheumatism, and that this occurrence in old persons often excites apprehensions of approaching paralysis. I have known old gentlemen so alarmed by it, as to seek medical advice; and as this affection sometimes precedes gout, and sometimes accompanies rheumatic arthritis and phlegmasia dolens, it is a fact worthy of your attention, and one which I would recommend you to hold in memory, though I must confess I am not able to give any explanation of it. I have seen an attack of this peripheral paralysis in a gentleman of gouty habit, and heard him express a great deal of surprise when he was told by Mr. Kirby, his medical attendant, that it would usher in a fit of his complaint. This gentleman, however, after taking some warm stimulant medicine, went to bed, and next morning had a regular attack of gout.

But to return to our subject. If you make experiments by handling snow, or immersing your hands in freezing mixtures, or any fluid of very low temperature, you find that, after some time, the exposed parts lose first the power of sensation, and afterwards that of motion, and that in this way you produce a complete, though temporary, local paralysis. Of this fact you are all aware. But what bears more strongly on the subject in question is that the paralysis, thus induced, is not merely confined to the hands and fingers, but also extends to other parts. You not only have the hands and fingers numb, but also lose, in a great degree, the power of flexion and extension, which is seated in the muscles of the fore-arm, and the motions of the wrist-joint are imperfectly performed. Now all this time the muscles of the fore-arm, lying at a considerable depth, and covered by warm clothing, are protected from cold, and yet you perceive they partake in the paralytic affection of the exposed parts. Here, then, is another example of the same nature, corroborating our former position, that causes producing loss of power in one part of the extremities of the nervous system may have not merely a local influence, but also travel towards the centre and affect distant parts.

Speaking of the influence of cold on the system, I have to observe that, from the experiments made on this subject by Hunter, Edwards, Dr. Marshall Hall, and others, some instances of its effects seem very singular. One of the most remarkable is the production of paralysis, which, in most cases, is partial, but is sometimes very general without being followed by death.

I remember the case of a dog, which lay buried in snow for two days, and was then taken out quite stiff and insensible, and thrown on a dunghill as if dead. After some time the poor animal gave some symptoms of reanimation, and finally recovered. The influence of cold has been alluded to by Dr. Abercrombie, and you will find that he mentions a case of paraplegia, arising from paralysis, brought on by cold, which lasted for eight months. A blast of cold air on one side of the face has been known to cause paralysis and distortion of several months' duration.

Again you have, as in the case of a man in this hospital, paralysis of the lower extremities from exposing the feet to cold and wet, while employed in bailing out water in a quarry. You may have observed the same thing brought on by similar exposure in fishing or snipe shooting, and that such causes gave rise to paralysis not only in the parts subjected to the influences of diminished temperature and wet, but even extended to the nervous centres, so as to produce decided paraplegia. I was once myself exposed to a very intense degree of cold on board a ship, and observed that the sailors who had been most exposed suffered severely, and did not recover from its effects during the rest of our voyage. In fact, many months will often pass away before the symptoms arising from cold are removed, and you will find that, in addition to the case of paraplegia from cold which lasted eight months, Dr. Abercrombie mentions another in which the paralysis was permanent.

One of the most remarkable examples of disease of the nervous system commencing in the extremities, and having no connexion with lesions of the brain or spinal marrow, was the curious *epidémie de Paris*, which occurred in the spring of 1828. Chomel has described this epidemic in the 9th number of the *Journal Hebdomadaire*, and having witnessed it myself in the months of July and August of the same year, I can bear testimony to the ability and accuracy of his description. It began (frequently in persons of good constitution) with sensations of pricking and severe pain in the integuments of the hands and feet, accompanied by so acute a degree of sensibility, that the patients could not bear these parts to be touched by the bed-clothes. After some time, a few days, or even a few hours, a diminution or even abolition of sensation took place in the affected members, they became incapable of distinguishing the shape, texture, or temperature of bodies, the power of motion declined, and finally they were observed to become altogether paralytic. The injury was not confined to the hands and feet alone, but, advancing with progressive pace, extended over the whole of both extremities. Persons lay in bed powerless and helpless, and continued in this state for weeks and even months.

Every remedy which the ingenuity of the French practitioners could suggest was tried, and proved ineffectual. In some, the stomach and bowels were deranged, and this affection terminated in a bad state of health, and even in death; in others, the vital organs, cerebral, respiratory, and digestive, were in the same state as before their illness, and their appetites were good, but still they remained paralytics. At last, at some period of the disease, motion and sensation gradually returned, and a recovery generally took place, although, in some instances, the paralysis was very capricious, vanishing and again reappearing.

The French pathologists, you may be sure, searched anxiously in the nervous centres for the cause of this strange disorder, but could find none; there was no evident lesion, functional or organic, discoverable in the brain, cerebellum, or spinal marrow. Now, here is another remarkable instance of paralysis creeping from the extremities towards the centre; here is a paralysis affecting all parts of the extremities as completely as if it had its origin in the central parts of the nervous system, and can any one, with such palpable evidence before him, hesitate to believe that paralysis, or even hemiplegia, without any lesion of the brain or spinal cord, may arise from disease commencing and originating in the nervous extremities alone?

I may observe, *en passant*, that where paralysis simultaneously attacks the arm and leg of the same side, it arises from an impression on the nervous centres; but this I think does not hold where the paralysis is creeping, as in the case before me, which has been reported by Mr. Hudson, and was under the care of Dr. Stokes. "The patient, James Moore, was admitted on the 4th of March, labouring under paraplegia, which he attributed to cold and wet. About a month before admission he first perceived a stiffness of the great toe of the right foot; afterwards numbness and coldness of the sole, and then of the leg as far as the knee, and dragging of the limb in walking. During the progression of the disease up along the thigh, it commenced in the left foot, and, after a few days, he experienced almost complete paralysis of sensation in the right lower extremity, and a lesser degree in the left, accompanied by so much diminution of the power of motion as to render him unable to walk without support. About three weeks after the appearance of paralysis in the lower extremities, the little finger of the right hand was attacked with numbness, which passed successively to the rest, attended with some loss of the sense of touch, and power of grasping objects. He has also had retention of urine, and the bowels were obstinately constipated. There was no tenderness of any part of the spine. He had no pain in the head. His pupils were natural, mind unaffected, pulse, sleep, and appetite also natural." Here, gentlemen, you have an instance of what I would term creeping paralysis, having its origin evidently in an affection of the peripheral extremities of the nerves.

I may now observe, that I have brought forward instances to prove that direct injury of one part of the nervous system may produce paralysis in another and distant part, but have we not also other instances? Certain substances, which produce morbid affections on the nervous system, are found to be attended with results analogous to those described. You are all aware that lead frequently brings on paralysis; that this is caused by the local application of lead, and that the effect of the local application extends chiefly to those parts to which the lead is directly applied. Thus, in painter's colic, the paralysis almost invariably begins in the hands and wrists, preceded, I will allow, in many cases, by symptoms of poisoning of the system, as shown by the tormina and affection of the intestinal canal. Dr. Bright has remarked, that in painter's colic the spine is frequently tender in the cervical region, when the upper, and in the lumbar, when the lower extremities are affected. It has been remarked, that spinal tenderness is often the consequence of disease of the extremities, and not the cause; so I think it is in painter's colic.

We found in this hospital a great number of cases in which there was paralysis of the upper extremities, without any spinal tenderness in the commencement; but when the disease had lasted for some time, the affection seemed to spread towards the spinal column. When this took place, it generally caused an aggravation of the disease; but it is no less true that we had many instances where it could not be discovered; and you are not to think that this irritation of the spinal cord should always precede the paralytic affection of the wrist and hand which is observed in painter's colic. You have seen in this hospital two cases of spinal tenderness supervening on peritonitis and acute gastric irritation, and, in fact, in every disease in which the nervous extremities, which are distributed to the parietes or viscera of the abdomen, are engaged, you find almost invariably that, after some time, there will be pain and tenderness of the spinal column as the consequence of these diseases. On the other hand, I grant that as soon as the spine becomes affected, whether the disease be tympanitis, peritonitis, or that swelling of the belly to which the name of hysterical meteorism is applied, there will be certainly an aggravation of the existing symptoms.

You perceive this conducts us to the solution of the question, how far, in the treatment of chronic complaints, are we to consider spinal neuralgia as the cause or consequence of the disease? Sometimes those troublesome hysterical affections which you are called on to treat are preceded by spinal neuralgia, but in many well-marked cases it is totally absent. I wish to call your attention to this subject, because medical men have been biased to a very considerable extent, by the statements made by Mr. Teale and others, respecting the treatment of various anomalous affections supposed to be connected with irritation in the spinal column. Every female who complains of any kind of abdominal or pectoral symptoms of an obscure nature is examined all over the spine, and if the slightest tenderness be detected, according to the practice generally pursued, you are to leech and blister her back, or to apply tartar emetic ointment.

I think I have seen injurious effects from this plan of treatment. Inquire carefully into the history of the case, and ascertain, if possible, whether it was the central or circumferential parts which were first affected, for, in the latter case, you can promise yourselves less from any local application to the spine than in the former; whereas, in those instances where the disease has travelled from the centre to the circumference, you may hope for success from local applications. It is important to recollect, gentlemen, that violent enteritic affections may produce paralysis of the lower extremities. In the case of a young gentleman whose disease arose from obstruction in consequence of eating nuts—and to which I shall advert in a future lecture, violent enteritis and peritonitis arose, and he had two relapses; from these he recovered with difficulty, but they left him paralytic of his lower extremities. After two months, the paralysis speedily yielded to the application of stimulating liniments. This case Mr. Kirby and Mr. Cusack saw. In another remarkable case, concerning which I was consulted by Dr. Ireland, a frequently recurring vomiting was in the end followed by paralysis of the lower extremities.

What I wish to impress upon your attention is, that pain, numbness, spasm, and loss of power from an affection of the circumferential parts of the nerves may commence in these extremities, and be propagated towards the centre, so as to be finally confounded with diseases originating in the central parts themselves. You have seen in the patient, James Moore, hemiplegia, which I am convinced had its origin in the extremities. Have you not also seen, in the cases of peritonitis, gastric irritation, and painter's colic, a consecutive affection of the spine? Indeed, it frequently happens that paralysis, commencing in the nervous extremities, may not only induce disease of the spine, but in time bring on disease of the brain itself. It does not follow that a fatal paralysis affecting the brain should commence in that organ. In Dr. Woolaston's case, are we to account for the occasional partial amaurosis under which he laboured for such a length of time before his death, by referring it to disease of the brain? In consequence of a temporary paralysis of one half of the retina of each side, he saw but the halves of objects, and from this he argued that there was a semi-decussation of the optic nerves. This happened several times, but never remained any length of time, and I do not think that at that period it was proved that any disease existed in the brain.

Some time back I saw, with Dr. Brereton, a very singular example of defective vision in a wealthy bookseller, who had lost the sight of one eye from accident. This gentleman, one day, in going up a hill near Clonskeagh, remarked that where there was but one man he saw two men, but divided at the middle, as if they were cut by a vertical line into two halves. I questioned him closely on the occurrence, thinking it to be the effect of imagination, but he said this was not the case, and that he was perfectly convinced he saw double. This is but one way of accounting for this optical delusion. It is well known that when vision is much impaired, the power of seeing light often remains, when the eye cannot distinguish any particular object. A partial and temporary paralysis of the retina, in a vertical section, may have given rise to an apparent white line bisecting the object vertically. Again; in the case of a fine young lady, whom I saw along with Dr. Beatty, amaurosis—acute, sudden, and complete — came on without any headache or cerebral symptoms being complained of. When called on to see her, I found her walking about the drawing-room, quite cheerful, and enjoying a good appetite, but perfectly blind. After the lapse of some days, these symptoms were followed by profound coma and death.

But there are other instances more decidedly corroborative of the positions I have laid down. You all know that if a man gets a blow or cut on the forehead, which wounds or divides the frontal nerve, not only the parts which that nerve supplies become paralytic, but that also the diseased impression thus produced spreads towards the centre, affects those nerves which anastomose with the frontal, and, by means of the communication formed between the nerves of the eye-ball through the lenticular ganglion, deranges the functions of the optic nerve, and causes amaurosis. Formerly I was in the habit of giving a different account of this, and thought that because, in some of the lower classes of animals, as for instance the mole, the fifth nerve, from which the frontal is derived, is the true nerve of vision—those animals having no optic nerve,* I had found an analogy capable of giving an explanation of the fact, that injury of the frontal nerve is sometimes followed by blindness. But this, I am of opinion, cannot be the true mode of accounting for the amaurosis, as I can now readily conceive how injury of any other nerve, having communication with the optic, may spread inwards, and finally derange or destroy its functions.

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* A curious instance of the total absence, or imperfection, of a pair of nerves, is related by the Rev. Mr. Bree, in the *Magazine of General History*:—"A white cat, of the Persian breed, was kept in his family as a favourite. The animal was a female, quite white, and perfectly deaf. She produced, at various times, many litters of kittens, of which some were quite white, others more or less mottled, tabby, &c. But the extraordinary circumstance is, that of the offspring produced at one and the same birth, such as were, like the mother, entirely white, were, like her, invariably deaf; while those that had the least speck of colour on their fur as invariably possessed the usual faculty of hearing."

You will frequently observe persons in the decline of life, who otherwise enjoy tolerable health, exhibiting, as it were, a slight shade of paralytic affection of the system, fitful and capricious in its appearance and duration, sometimes remarkable on every instance of corporal exertion, sometimes scarcely at all, presenting at one time a reiteration of successive attacks, and at another time being totally absent for months. Some cases of this kind I have studied for months, and one in particular for years. The gentleman, who was the subject of the latter, complained of barely perceptible weakness and dragging of one of his legs whenever he was tired; but if he took a glass of wine on coming home, he got quite well, and these symptoms disappeared. Matters went on this way for a considerable length of time, the paralysis being at one time in one leg, and then in the other. At last he got a paralytic stroke, which lasted for some time and then subsided. He next got confirmed paralysis of one side, and soon after this was carried off by an attack on the brain.

You will often find persons similarly affected with paralytic attacks of the extremities, at first slight and transient, but afterwards increasing in vigour and intensity, until they terminate in ramollissement or effusion. Formerly I was of opinion, that this fugitive and shifting paralysis depended upon local congestion in the brain, and others have attributed it to effusion, but this is not the fact. Persons may die after having laboured for some time under hemiplegia, and yet no trace of lesion of the cerebral mass be detected: and why? Because many of them are cases of this creeping paralysis, commencing in the peripheral extremities, and travelling gradually towards the centres of the nervous system.

It is only on the principle of there being such a disease as local paralysis not induced by lesions of the nervous centres, that we explain the origin and nature of such cases as paralysis of the deltoid, concerning which Dr. Elliotson has made so many interesting observations. *It is by reference to this hypothesis alone that we can account for the following cases*, detailed by Dr. Cooke in his admirable work on palsy:—

"I have lately had an opportunity of seeing a case of anomalous hemiplegia attended with circumstances not less extraordinary than those above described. An officer of high rank in the army, who is now about 60 years of age, was, in the year 1795, affected with a diminution of power in the right hand. This complaint increased, notwithstanding a variety of modes of treatment, till the year 1800, when, after a course of mercury, recommended by Mr. Cline, its further progress was stopped, since which time the disease has remained stationary. The peculiar circumstances of this case are the following:—The muscles of the left arm, from the shoulder to the elbow, are much wasted, and greatly diminished in power; while the muscles of the fore-arm are not at all lessened in size, and but little in power. The state of the right side is just the reverse, the muscles of the upper arm being of their natural size, and possessing their full power; whilst those of the fore-arm are very much wasted, and their motion, especially that of the fingers, is almost entirely abolished. In all other respects this gentleman appears to be perfectly well. No cause for this disease can be assigned, nor did any method of treatment afford the smallest relief, till the mercurial course was adopted, when the progress of the disorder was arrested in the year above-mentioned. Since that time no attempts to remove the complaint have been made, yet it does not increase.

"In a late publication by Mons. Keratry, a case of general palsy is related, the circumstances of which are very extraordinary. This case is adduced with a view of showing how little residue of animal existence is sufficient for the preservation of the intelligent being. There is now living, he says, in D'Isle et Vilaine, a person who, after having been blind for ten years, lost also the sense of hearing, and in a little time afterwards became almost universally paralytic. He was entirely deprived of the use of his arms, legs, thighs, and of the whole exterior surface of the body, with the exception of a part of the face; but the power of speech, and the functions of respiration, circulation, and digestion remained. Under these deplorable circumstances, however, he is not, says Mons. Keratry, wholly without consolation, for a sort of intercourse is preserved with his family and friends, by means of characters traced on that part which still retains its sensibility, and in this state of unexampled misery he retains, in some degree, the distinguishing character of man—intelligence."

I saw, with Sir Philip Crampton, a case of paralysis, in which the mouth was drawn upwards and to one side, accompanied by examine the march and progress of this paralysis, we find it attended with many circumstances clearly denoting its origin in the nervous extremities, notwithstanding what some French pathologists have asserted to the contrary:—the slow manner in which it creeps from one part to another; the fact that, after the disease has occasioned an almost complete loss of power in the lower extremities, the weakness may, on some days or hours, be less remarkable or even disappear altogether; so effectually indeed, that if, for experiment, you endeavour to throw the patient down, he will give very powerful resistance. In this circumstance, says Andral emphatically, we find the proof of the absence of any organic lesion.

Another proof of its not depending on any lesion of the nervous centres is derived from the very extent to which it may arrive; for, in the third stage of the disease, the paralysis is complete and general, including the four limbs, the tongue, and the voluntary muscles of the trunk. The involuntary muscles, too, especially those connected with the respiratory movements, become influenced; in this third and highest degree of the paralysis, convulsive movements may also occur, presenting the strange phenomena of the alternate paralysis, and the complete contractility of the same voluntary and involuntary muscles, and of a voluntary muscle, which is perfectly disobedient to the will, being thrown into bizarre and unwonted motion by the involuntary impulse. This fact, gentlemen, is in itself sufficient to prove the truth of the proposition I have advanced, that a morbid state of the nervous extremities is often unconnected with, and independent of, any central lesion.

In my own practice, cases of creeping paralysis corroborating this conclusion have occurred. Thus I saw, in consultation with Mr. Colles, a clergyman, all of whose extremities had gradually become affected with the slightest possible degree of paralysis, affecting both the motion and sensation, the latter rather more than the former. The progress of the disease was so irregular and gradual, it is so variable, and has now lasted so long without any further increase in its intensity, that both Mr. Colles and myself have little doubt that the disease is unconnected with any lesion of the brain or spinal marrow.

In the following interesting case the paralysis is also evidently independent of any alteration in the nervous centres. Dr. Knaggs of Mountrath had a very severe and prolonged attack of the late epidemic fever in the month of March, 1848; his life was much endangered, his head being engaged throughout, but he had no apoplectic nor convulsive fit. On recovery he found that he had almost entirely lost sensation in the ring and little fingers of the left hand, but the power of motion was complete: while in the forefinger of the same hand there was paralysis of motion, but sensation was perfect. This state continuing, he came to town a month afterwards to consult me. When I saw him, with Dr. Neligan, the paralysis of sensation and of motion was just as when he first experienced it, but he thought that he had less power in performing any delicate manipulation with the forefinger, and there was very great atrophy of all the *special* muscles of this finger, while the other muscles of the hand and arm, including those of the ring and little fingers, were not in the least wasted: thus affording a beautiful illustration of the *intimate connexion which exists between the motive power and nutrition.*

Before concluding this summary of my views on some points connected with the Pathology of the Nervous System, which I published for the first time many years since, and of the truth of which subsequent experience has fully convinced me, I cannot avoid expressing my surprise that Dr. Todd—in his admirable essay on the Pathology of the Nervous System, published in the *Cyclopædia of Anatomy*—has not noticed my observations, although it is evident from the following paragraph among others that he has arrived at the same conclusion:—"I shall here cite various facts in addition to those already adduced, which unequivocally demonstrate that a power exists in the cord of exciting movements in parts which receive nerves from it, by changes occurring in its substance, which may arise there from some modification of its nutrition developed in the cord itself, *or be excited by a stimulus brought to act upon it by afferent or sensitive nerves.*"

LECTURE XXXIV.

APOPLEXY.—PATHOLOGY OF CEREBRAL DISEASES.

GENTLEMEN,—Two persons labouring under severe cerebral disease, admitted lately into the same ward, presented a striking contrast between the symptoms by which each respectively was accompanied; in fact, so completely did these cases differ in their duration and history, that they scarcely resembled each other in anything but their fatal termination; and it was consequently expected by all who had watched their progress during life, that an examination of the brain would detect lesions of that organ as different in their nature as had been the symptoms which they had occasioned.

Such, I confess, was my own opinion, and such was the opinion of many others who have no little experience in pathology. The result, however, differed widely from our expectations, and is therefore well worthy of your attention.

As this result is in direct opposition to our preconceived opinions concerning the origin and causes of some of the most serious derangements of the cerebro-spinal functions, I must trespass on your patience while I lay before you the particulars of these cases, and the lesions observed on dissection; after which we shall compare them together, and consider what pathological and practical inferences may be drawn from them. I am more anxious to draw your attention to this subject, because many late writers on diseases of the brain affect an accuracy of diagnosis which I have found unattainable in my practice. Numerous cases, it is true, are cited by each of these authors, and are so arranged and classified that the conclusions seem to be arrived at by a perfectly fair induction, and of course command our assent on the strongest grounds, the evidence of facts. It is to be feared, however, that these facts have been too frequently warped to suit preconceived pathological arrangements, apparently founded on the basis of morbid anatomy; and

I am inclined to think that a more unbiassed observer will find little cause to join the ranks of those who claim for this department of medical science a degree of accuracy almost equal to that which the unrivalled discoveries of Laennec have enabled us to attain in the diagnosis of pectoral affections. To prevent the suspicion of having accommodated the history of these cases to any opinion of my own, I shall read them out from the case-book.

Patrick Kearney, aged forty, admitted October 6th.—Has always enjoyed good health, with the exception of being subject occasionally to ill-conditioned ulcers. Three months ago, after having been subject to very violent vertigo for some time, he was attacked by slight hemiplegia of the left side, from which he recovered in three days. The vertigo, however, continued, and in walking he consequently frequently staggered, and sometimes fell, but did not become insensible; and on such occasions he was able immediately to rise from the ground without assistance. Three weeks ago he again lost the use of his left side in the evening, and says that this attack was not preceded by headache. His left arm has lost the power of motion, but not of sensation. The fore-arm is flexed on the arm, the fingers on the hand, while the latter is bent towards the fore-arm. Extension of these parts could not be effected, even by the application of considerable force, and every such attempt appeared to give him pain. This flexed state seemed to arise from a permanent tonic spasm affecting the flexor muscles of these parts; and it is remarkable that it continued even when the patient was asleep. He has occasionally great trembling in this limb, but no pain. The left lower extremity is less engaged; there is no flexure, and but little trembling. Pulse 92, full and soft; other functions natural.

His disease underwent no material alteration until eleven o'clock in the forenoon of the 15th October, when his respiration became suddenly stertorous, and his eyes fixed. The stertor increased, and in about ten minutes he became quite comatose, having lost all power of sense and motion, and his limbs were stiff. This fit lasted about half an hour, and on its subsiding he recovered his consciousness perfectly, but his voice was very obscure, and his articulation difficult. His whole frame, too, continued to be agitated by a nervous restlessness and tremor.

In the evening he had another fit, which was not so severe as that of the morning. During the night he did not sleep a moment, but constantly cried aloud, so as to disturb the other patients, and was perpetually agitated and restless, making frequent attempts to leave his bed. At eight a.m. on the 16th, the hemiplegia was observed to be increased, while the tonic contraction had extended to the left lower extremity. During the visit, a continued shivering affected him generally, but it seemed greater on the affected side. This rigor soon subsided. Although so agitated and restless, and although he was constantly crying out in an incoherent manner, as if from pain, yet when spoken to he answered in a perfectly rational manner, and said he had no pain in the head, nor did he lose his intellect or speech until the very moment of his death, which took place about noon on the same day. During the time which intervened between the first fit and his death, the pulse and heat of skin are noticed to have continued as before.

Examination of the body 18 *hours after death.*—Cadaveric stiffness inconsiderable; contraction of the left leg resolved, that of left arm remains with considerable stiffness. The vessels of the scalp contained but little blood, but on opening the cranium the sinuses of the dura mater were found much distended by fluid black blood. The vessels of the pia mater exhibited an intense congestion, being everywhere distended with dark-coloured blood. No blood was extravasated on the upper surface of the brain, neither was there anywhere a trace of sub-arachnoid serous effusion, or of puriform matter, coagulable lymph, &c. At the base of the brain a stratum of extravasated blood, in some parts very thin, but in other places two or three lines in thickness, was found at both sides of the *pons*, and occupying all the space between it and the commissure of the optic nerves; coagulated blood also existed in the fourth ventricle, and, passing by the *iter*, it so exactly occupied the third, and both lateral ventricles, that when extracted the coagula appeared like casts of these cavities. It is to be observed, however, that the blood so effused into these cavities by no means considerably distended them.

A pretty accurate account of its quantity in all may be formed from the fact, that in each of the lateral ventricles the coagulum in size and shape resembled a leech of the ordinary size, when affected with a pain which shot across the epigastrium from one hypochondrium to the other, and rendered stooping at his work extremely distressing. The patient was considerably emaciated; appetite good; some thirst; tongue red and dry; bowels free, two or three stools being passed daily; no tenesmus; involuntary discharge of urine; no pain or tenderness in the region of the bladder; pulse 120; sleeps well; has no pain in the head; no derangement whatever of cerebral or respiratory functions; his eyes are suffused, but not weak or sore. Twenty leeches were applied to the epigastrium, and he was put on low diet.

November 7th.—Nurse states that he continued without any alteration in his symptoms until yesterday evening after supper, when, becoming very drowsy, he went to bed, and fell into what she thought was a natural sleep. This morning, however, she became alarmed at finding that she could not awake him. He is now lying in a state of deep coma, and constantly tosses his head from side to side on the pillow; the eyes are suffused; the pupils dilated, and totally insensible to light; there is slight strabismus of the right eye. Skin warm; pulse 120, hard, and somewhat full; a rale is audible in the trachea. A vein was immediately opened, but when about three ounces of blood had been taken, the pulse became very weak, and he appeared so sunk that no more blood was drawn. The pulse shortly after regained its strength, and the tracheal rale ceased. An injection of several pints of warm water was carefully administered by means of Read's syringe, and brought away an enormous quantity of hardened feces. In two hours a turpentine injection was ordered. In the meantime his head had been shaved, and was kept constantly wet with towels dipped in cold water, while the actual cautery was applied to the nape of the neck, and a scruple of calomel was given, to be followed in the course of the day by a draught containing castor oil and spirits of turpentine, for the purpose of removing or diminishing the tympanitic state of the belly, which still persisted. None of these measures afforded him the least relief. The draught was no sooner swallowed than it was rejected, and the application of the cautery roused him but for a few minutes, after which he again became comatose. In the evening he had a severe fit of screaming; his pulse rose to 140, was somewhat full and hard; and his death, which took place about nine o'clock that evening, about twenty-six hours from the first appearance of the cerebral symptoms, was preceded by two or three slight convulsive fits.

Dissection 12 hours after death.—Head:—There was no congestion of the vessels of the scalp; on removing the calvarium, the sinuses of the dura mater were found gorged with black blood, mixed with small quantities of fibrine deprived of colouring matter. No fluid was found between the visceral layer of the arachnoid membrane and the convex surface of the brain, and not more than a teaspoonful at its base. The pia mater was excessively congested, its larger veins gorged with black blood, and their smaller branches, similarly distended, formed numerous ramifications over that membrane. In the ventricles of the brain was a small quantity of serous fluid, and a little in the third ventricle, but the quantity of serum so effused was too inconsiderable to be considered as a morbid product. The substance of the brain and cerebellum was perfectly healthy in every respect. In both this and the preceding case the brain, when cut, exhibited numerous red points, but not more than are frequently seen on the section of a perfectly healthy brain.

Thorax:—Nothing remarkable, except a considerable engorgement of the posterior portion of both lungs, owing partially to cadaveric gravitation, and partially to the effect of gravitation during the long agony preceding death. This, from affording a crepitating rale before death, and from its rendering the pendent portions of the lung impervious to the air, Laennec has termed *the pneumonia of the dying*, a term by no means applicable, for pneumonia renders the pulmonary tissue impervious, in consequence of an exaltation of the vital powers of the affected part; whereas, in the impervious pulmonary tissue just spoken of, this state arises from a decrease, a gradual cessation of the vital powers, which permits the vessels to allow the blood, in obedience to physical laws, to accumulate in the most depending part.

Abdomen:—The large intestines were flaccid and empty, and lay concealed beneath the stomach and small intestines, both of which were excessively distended with air, and presented on their serous surfaces the appearance of intense venous congestion; the veins, everywhere gorged with dark blood, were injected with this fluid to their ultimate ramifications. There was a considerable congestive redness in the mucous membrane of the stomach, and that of the small intestines was throughout their whole extent of a slate colour, evidently produced by its state of sanguineous engorgement during life; the most pendent portions of the intestinal loops were red, and still more congested, in consequence of *post-mortem gravitation.*

Having thus put you in possession of the symptoms and post-mortem appearances observed in these two cases, I shall now, gentlemen, proceed to compare them together, and afterwards examine them with reference to the opinions expressed by writers on diseases of the brain.

In the first place, no two cases could possibly differ from each other more than these in their duration, general history, and individual symptoms. In one, coma suddenly supervened without any previous warning, and persisted until death, accompanied by dilatation of the pupils, and insensibility of the retina to light. Here the derangement in the sensorial functions was quite unexpected, and there were neither hemiplegia, tonic spasms, rigors, nor successive fits of convulsions, which were the very symptoms that in the other case constituted the chief features of the disease. In the other case, too, were absent the uninterrupted state of coma, the contraction of the pupils, and the insensibility to light. The state of mind in each was strikingly different; in the one, being as it were annihilated from the very commencement, while the other patient answered questions rationally to the last. In the old man the cerebral affection had subsisted for several months; in the young man it had proved fatal in twenty-four hours.

Having formed a general comparison between the symptoms of these two cases, can we, in the lesions observed in the examination of the brain, detect the causes of the numerous and striking differences just enumerated? Most certainly not, for the morbid appearances were exactly the same in both, if we except the blood effused on the base and in the ventricles of the old man's brain. Arguing from the generally received ideas concerning the effects of such an effusion of blood, its detection in these situations would undoubtedly lead the morbid anatomist to conclude—had the bodies of both these patients been presented to him for examination—that the man in whose brain this effusion had occurred must during life have been much more likely than the other to present such symptoms as permanent coma, dilatation of the pupils, insensibility of the retina to light, &c. In fact, it is quite obvious that the post-mortem appearances would mislead him, and that the history of the cases thus formed would be extremely incorrect—symptoms being attributed to one which had only been displayed by the other. I do not mean to assert that morbid anatomists have not long ago observed that coma, dilatation of the pupils, &c., may occur without effusion, or that effusion may exist without having occasioned these very symptoms. Still, however, it cannot be denied that the cerebral mass and membranes being found in every other respect in exactly the same state in the two cases, an effusion of blood on the base and in the ventricle of one being superadded to the appearances observed in the other would be considered as constituting an important difference, increasing the probability of the occurrence of coma, &c., during the life of that patient. The cases just related exhibit striking exceptions to the justice of such a mode of argument.

Let us next, gentlemen, compare these cases with the opinions recorded by authors concerning the lesions connected with certain symptoms.

No proposition seems more universally allowed by those who profess to reduce cerebral diseases to a classification depending on evident alterations of structure, than that paralysis of one side of the body always arises from a local affection of the opposite hemisphere of the brain. This affection may either consist of an effusion of blood, a *ramollissement*, or the pressure arising from a tumor, &c.; but in all cases it is assumed that hemiplegia must be attended with and caused by some such local and evident alteration. On the other hand, general paralysis, affecting alike both sides of the body, is caused, according to most authors, by a general derangement of the cerebral circulation usually called congestion, and believed to act equally on both hemispheres. The latter species of paralysis may arise suddenly, and may be as suddenly relieved, as it ceases when, by means of venesection, we succeed in removing the congestion that produced it.

An unbiassed attention to facts will, I think, prevent us from giving our assent to either of these propositions. In the first place, we often, in dissecting the brains of hemiplegic patients, find both hemispheres, so far as evident alteration of structure, affected exactly in the same way. This was remarkably the case in Kearney; there was no alteration in one hemisphere which did not exist in the other, and yet this man had complete paralysis of one side. It is in vain to assert that some alteration of structure existed, but escaped our notice, unless it were microscopic, for both myself and those who assisted in the dissection were too familiar with diseased appearances, and too careful in conducting the examination, to allow any difference in one hemisphere as compared with the other to escape notice. In the next place, it is by no means an unfrequent occurrence to meet with patients who, being suddenly attacked with symptoms of general determination of blood to the head—such as headache, tinnitus aurium, vertigo, are rendered for the time more or less completely hemiplegic, and yet recover in the course of a few minutes or hours the use of the affected side so suddenly and so perfectly, as to preclude the idea of local lesion such as could be detected by the scalpel of the anatomist. Of this I have seen several instances both in hospital and private practice, and which I cannot reconcile with the doctrines laid down by Rostan, Lallemand, and other authors.

To quote one of the many examples I myself have seen:—A man named Thomas Lynch was admitted into Sir Patrick Dun's Hospital, afflicted with symptoms indicative of cerebral disease. During his residence in the hospital he suffered four or five attacks of hemiplegia, in every respect complete, and depriving him of the use of his speech. Some of these attacks lasted only fifteen minutes, while the longest continued about an hour and a half: they ceased as suddenly as they commenced, and left no traces of hemiplegia behind them.

The circumstances of this case evidently prevent us from assigning each attack to a separate effusion of blood; for were it owing to this cause, it would be impossible to account at once for the sudden appearance and as sudden cessation of so extensive and complete a paralysis.

Again, I have carefully watched the progress of several cases, which after months and years have finally terminated in hemiplegia, the supervention of which I had anticipated from the patients having remarked to me, that although otherwise in good health, they had more than once observed, when fatigued by exercise, that they felt a degree of weakness in one leg, the motion of which, so long as this feeling continued, they described as slightly approximating to the dragging of a half-paralysed extremity. In some this feeling was accompanied by a scarcely observable thickness of speech, and a certain confusion of mind, all of which subsided shortly on their taking rest. These persons usually complained at the same time of numbness in some part of the affected extremity, and which numbness not unfrequently was the sole symptom of these transient warnings. The remark already made with regard to Lynch's case applies more strongly here; and since the hemiplegia, when it did supervene, always affected the side in which these premonitory symptoms had been felt, we can scarcely avoid attributing both to the operation of causes the same in nature but differing in degree.

Many, I am aware, would account for the transient attacks by supposing that each was preceded by a very small effusion of blood in the opposite hemisphere of the brain, and that the final complete hemiplegia was owing to a similar but more copious effusion. I am ready to admit the truth of this explanation in those cases where there have been several distinct attacks of paralysis, differing in intensity, all affecting the same side, and all lasting several days, or even weeks, and then *gradually disappearing.* Instances of this kind are frequent, and in such it is not unusual to find traces of those successive extravasations of blood which had caused the series of paralytic attacks; but the comparatively longer duration, and the gradual cessation of such attacks, sufficiently distinguish them from the affections above spoken of, and which are too sudden in their disappearance to admit of a similar explanation.

The manner in which the arteries of the brain communicate together renders it more difficult to conceive how local determinations of blood could occur in this organ. Still, however, such an occurrence is by no means impossible; and did it take place, it would account for the phenomena observed. Thus, were the right side of the brain to become congested, a sudden attack of hemiplegia of the left side of the body would be produced suddenly, and would as suddenly subside on the removal of that congestion. When the congestion is violent, and affects the whole hemisphere, the paralysis will affect the whole of the opposite side, and will be intense; when, on the contrary, the congestion is inconsiderable, or else confined to particular portions of a single hemisphere, the paralysis will be in proportion less severe and less extensive. This explanation* does not appear to be inconsistent with the laws known to regulate the circulating system in other organs, for it is by no means unusual for the parts deriving their blood from one common artery to display occasionally very different degrees of sanguineous congestion, a circumstance only explicable on what appears a very tenable hypothesis—an active participation on the part of the smaller vessels and capillaries in the process by which every part of the body is supplied with blood.

Another mode of explaining the occurrence of such attacks as I have described, is to suppose that they arise from a mere functional derangement, more or less intense, of the whole or a portion of one cerebral hemisphere. This explanation would certainly account for the sudden appearance and cessation, as well as for the short duration of such paralytic affections; but I do not feel inclined to adopt it, because they are invariably accompanied by other symptoms denoting determination to the head; and also because sooner or later they usually terminate in actual extravasation of blood in the side of the brain opposite to the side of the body affected by these transitory attacks. Whatever mode of explaining the occurrence of these latter be adopted, it is important, gentlemen, to recollect that whenever they are observed, the medical attendant must be on his guard —must warn the patient's friends of his future danger, and must endeavour, by the most suitable means, to avert the tendency to cerebral congestion, and its consequence, extravasation. It is to be regretted that the latter is too often inevitable; such cases, in persons past the prime of life, being usually attended with an alteration in the texture of the arteries of the brain disposing them to rupture.

The state of these vessels in Kearney was worthy of attention, as the existence of three coats or tunics, which some have denied, to the cerebral arteries was here demonstrated. Another symptom

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* Rostan has advanced this explanation under the head of "Congestion cérébrale locale"; but he does not attempt to account for the manner in which these local affections are produced, nor does he sufficiently dwell on them as the frequent precursors of paralysis from extravasation on the side of the brain most prone to these local congestions.

—tonic spasms of the affected side—formed one of the most remarkable features of this poor man's disease, and combined with the hemiplegia, seemed to furnish indisputable evidence of some local affection of the opposite side of the brain, and yet none such was detected; the congestion of the pia mater was intense on both sides, although somewhat greater on the side opposite to the paralysis. The difference, however, was inconsiderable, and might have been occasioned by the position of the head shortly before or after death. I do not say it was so, for the position was not observed, but I mention this explanation to impress on your minds how trifling was that difference. Here, then, is a second instance of an affection permanently confined to one side of the body, without any lesion to account for it being found in the opposite side of the brain,—a fact at variance with the testimony of several systematic writers.

The tonic spasm of the paralysed extremities requires notice in another point of view, as constituting one of the chief symptoms characteristic of *ramollissement*, or at least that state of brain which finally ends in softening. The absence of any local cerebral affection in Kearney, in whom this operation had been during life so remarkably developed, is conclusive in proving that even its most extreme degree may be excited by some other cause. The same remark applies to the headache, the tingling, and the spastic pains of the affected limbs, the paralysis, and in fact to each of the whole group of symptoms which are said, when combined with the tonic spasm, to constitute indubitable evidence of *ramollissement*. I do not deny that, when associated together in the order described by Lallemand and Rostan, *they afford very strong evidence of that lesion;* but this I will assert, that I have met with several cases in which, after a careful comparison of the symptoms with the descriptions of these authors, I was induced to make the diagnosis of *ramollissement* with considerable confidence, and yet, as the result proved, erroneously. Had such mistakes occurred in my own practice only, I might possibly have believed that I had not rightly understood these celebrated pathologists, but I have witnessed similar errors committed by others so often, that I am rather inclined to doubt the general applicability and correctness of the rules laid down for recognizing this lesion.

Let it not be imagined, however, that I wish to throw doubts upon the beneficial influence of morbid anatomy on the diagnosis and treatment of diseases of the brain; far be from me any such intention; my object in making these observations is not to retard, but to advance, the progress of morbid anatomy, by pointing out the errors of some generally received opinions, and thus opening the way for a renewed and unprejudiced examination of the subject. It may, indeed, be *a priori* expected, that of all organs the cerebro-spinal system must give rise to the greatest number of diseases which, without much impropriety of expression, may be termed functional, being of such a nature as to be unaccompanied by sensible changes in the matter of the diseased tissue, and consequently not entering within the province of morbid anatomy. We all know that *tetanus* may be artificially produced by irritation of the spinal cord, and consequently that inflammation reaching that part often occasions this disease. So far we obtain from morbid anatomy useful knowledge concerning the nature and treatment of certain cases of tetanus; but do we advance or retard the progress of this department of medicine, by asserting that inflammation of the spinal cord exists in every case of tetanus? So it is with those who, affecting to account for all cerebral diseases by lesions observed after death, have excited expectations in the student, which not being in every case fulfilled, he is tempted in disgust to abandon all further investigations on the subject.

No other organ of the body, in the healthy discharge of its functions, presents such opposite states as the brain during the period of being awake and asleep, and yet we may reasonably doubt whether these states are accompanied by any physical change in the brain or its appendages, of sufficient magnitude to be within the cognizance of our senses.

Can we perceive any physical alteration in the cerebro-spinal system of an animal suddenly killed by prussic acid, or by violent concussion? and yet both these undoubtedly act on the nervous system.

Nothing proves in a more convincing manner that morbid anatomy cannot be expected to reveal the nature of all cerebral diseases, as has been too implicitly taught by many French pathologists, than its being totally incapable of suggesting or explaining the action of some of our most useful remedies. Thus, what are the physical conditions of the brain in delirium which indicate, if known, the exhibition of opium? or, in other words, why does this medicine act so much more beneficially in *delirium tremens* than in other species of delirium? What physical change does the nervous mass undergo in *chorea Sancti Viti*, which would lead us to expect such decided advantage from the carbonate of iron? What alteration of nervous structure would induce us to try the effects of arsenic in certain cases of neuralgia, or of strychnia in paralysis from lead? Would the inspection of the brain of a person labouring under sea-sickness of itself be sufficient to prove that the only certain method of checking this vomiting is to replace the patient on *terra firma!* All these considerations, gentlemen, leave no doubt on my mind that the ancients were not so wrong as Rostan and others would have us believe, in thinking that many nervous diseases were unattended with appreciable organic changes in the nerves, or nervous centres.

The object of morbid anatomy, therefore, should be not to explain the causes of cerebral diseases, but to investigate and ascertain in what number of such diseases we may with confidence refer the origin of the symptom to evident lesions. I fear much that modern authors have not sufficiently attended to this distinction, and, consequently, have most injudiciously endeavoured to establish systems, embracing all the various diseases of the brain and spinal marrow, on the basis of morbid anatomy, a mode of proceeding injurious to the latter science, and little calculated to promote the interests of practical medicine. If other proofs of the truth of this assertion were wanting, I might appeal to the almost endless opinions lately published concerning the physical alterations of the brain supposed to produce insanity and its attendant diseases; opinions apparently supported by numerous dissections, but really too often resting upon the supposed existence of morbid appearances which are sought for with such avidity that they are *always found!*

The following case is another good example of the truth of the doctrine I have been now trying to enforce. It was one in which very long-continued epilepsy existed without any appreciable lesion of the brain or spinal marrow. Mr. A. B., the subject of the case, was visited during his long illness by a great many medical men; among the rest by Mr. Colles, Sir P. Crampton, Mr. Smyly, Dr. Lees, and myself. He died on the 27th December, 1839, aged thirty years.

He had been a very fine, robust, and intelligent boy until he was nine years old, when he unfortunately got possession of five or six hard, unripe pears, and devoured them greedily: in a few hours he became thirsty, and drank a large quantity of buttermilk; in the course of the evening he fell into a state of insensibility, during which he was convulsed; a physician of great experience and judgment from Kilkenny was called in, who opened the temporal artery immediately on seeing the patient, and employed the usual means resorted to on such occasions; notwithstanding this, the insensibility continued, and in about seven hours it was observed that a hard tumor could be felt distinctly in the epigastric region. This induced the suspicion of the presence of some undigested substance, and a strong purgative enema was therefore administered; its effect was most satisfactory, for after the discharge of some copious stools the tumor subsided, and the boy recovered his senses. The injury inflicted on the cerebral system by this violent shock manifested itself soon after in the recurrence of the fit, and from that time forth he was subject to epileptic attacks. They annually became more frequent and more severe, but the vigour of his intellect was not impaired until after the disease had continued six years, when his mental faculties displayed a manifest dulness, and in the course of a few years more he gradually lapsed into idiocy, with however occasional gleams of reason, particularly on subjects connected with religion.

He now remained entirely in the house, and for many years had several epileptic fits daily; the convulsive stage did not usually last more than three or four minutes, but the coma often continued nearly an hour. The disorder generally exhibited a manifestly increased severity twice a year, when the fits would return about ten times daily, and with more than ordinary violence; after such a paroxysm had lasted about a week, it invariably terminated in outrageous madness, the appearance of which was a sure sign that the paroxysm, so far as regarded the fits, was over; this madness was of the most violent and noisy description, and required restraint; when it had subsided, as it usually did in about three days, he relapsed into his ordinary state with a few and comparatively slight fits daily.

Such was the course of the disease for sixteen years, during which he was most tenderly and assiduously nursed. I ought to have mentioned that a sudden and copious bleeding from the nose often took place when a fit came on; the breathing was invariably violent, irregular, and heaving for eight or ten minutes after the convulsions had ceased, but then gradually became tranquil, and so continued for the remainder of the comatose stage. During the last five years of this gentleman's life the fits became gradually less violent, but never ceased; for several years before his death he remained free from the attacks of madness.

In 1838, he became subject to diarrhœa, which recurred frequently, was difficult to stop, and seemed to have induced a most depraved appetite; in fact, at certain times he would swallow everything he could lay hold of, paper, coals, cork, lead, glass (after the mastication), boxes of family pills, straw, bits of books, &c., &c., from none of which did he seem to sustain any permanent injury. These fits of depraved appetite used to come on at irregular intervals; about 1838 he began to fall away in flesh, and for the last few years was pale, haggard, and emaciated. His sleep was, however, sound, and his appetite usually normal. About two months before his death the bowel complaint returned with more than its usual violence, and soon weakened him so much, that for the first time from the commencement of his illness he was confined to bed, and everything failed to check the diarrhœa, which finally proved fatal, exhibiting during its progress the usual symptoms of chronic inflammation succeeded by ulceration of the mucous membrane of the intestines.

While the diarrhœa was on him, and indeed all through his illness (except perhaps during the convulsions), *his pulse was perfectly natural, slow, and soft*, and so continued to within two days of his decease. The respiration (with the exception formerly noted) was always perfectly natural; never in the least short or hurried, and he never had a cough until two nights before he died, when he had a violent fit of coughing which lasted a quarter of an hour, and was apparently stopped by a dose of hartshorn in water; the same happened on the following night. He was never observed on any occasion to expectorate, and never had a vestige of wheezing in his chest; in fact, he was in all appearance so free from the least suspicion of pectoral complaint, that neither I nor any one else had examined his chest for many years. It is true that ever since the first epileptic seizure he frequently complained of what he termed pain in his heart, and nineteen years ago he was blistered for it by Dr. Ryan, of Kilkenny. This pain, referred invariably to the left side, used often to go away for considerable intervals, and was consequently believed to arise from a straining produced by the violence of the convulsions; during the last year of his life this pain was very constantly complained of. About three years ago I saw him for jaundice, which lasted about three weeks, and disappeared without medicine; I could not, at the time, make out the cause of the jaundice; he had no pain, no fever, no hepatitis.

The preceding history of my patient's case is imperfect, but as far as it goes its accuracy may be relied on. I am particularly anxious to impress this on your minds before I relate the result of the *post-mortem* examination, which was conducted under the most favourable circumstances, and at the express wish of the family of the deceased, by Dr. Lees and Mr. Quinan, in the presence of myself and Mr. Smyly; we had the advantage of a well-aired and admirably lighted room, and during the dissection the morning sun shone brightly on each organ in succession as we examined it; I mention these facts, lest any one should hereafter attempt to explain away the extraordinary discrepancy which this case exhibited between the symptoms observed during life and the morbid appearances discovered after death; the dissection was slowly and carefully conducted, and occupied five hours.

The following account will prove that, except ulceration of the bowels, we found nothing we expected, and many things totally unexpected.

Post-mortem examination of Mr. A. B. twenty-four hours after death.—Body emaciated to an extreme degree: the scalp, cranium, dura mater, arachnoid, pia mater, together with the cortical and medullary substance of both cerebrum and cerebellum, all perfectly healthy; a very small quantity of transparent serum was found in the ventricles; there was no notable subarachnoid effusion. The spinal marrow and its investments were quite normal.

The *pleura pulmonalis* of the right side was everywhere intimately adherent to the ribs; *the right lung itself was rendered quite solid by tubercles*, which occupied its whole structure, and

LECTURE XXXV.

DELIRIUM TREMENS.—CHOREA.—EPILEPSY.

LET me first, gentlemen, direct your attention to the case of a man above stairs, who had such a complication of affections that it is quite impossible to give his disease a name. He is, in fact, a kind of synopsis of the phlegmasiæ. You have seen him in one of the upper wards, a careless, idle, drunken vagabond, but possessed of a constitution naturally good. He had within the last few days delirium tremens, he had herpetic eruption on the face, he had violent bronchitis, severe pneumonia, inflammation of the mucous membrane of the stomach and bowels, inflammation and enlargement of the liver. Here was a complication of diseases extremely hard to treat. Such a complication exemplifies the advantage to be derived from general treatment. From an attentive consideration of the manner in which they arose, we were enabled to treat in a proper manner, and overcome these diseases.

In the first place, this man was a person of intemperate habits; he had walked about the city for two days and two nights in a state of drunkenness, exposed to rain and cold. The inflammations by which he was attacked set in simultaneously, or, at least, we cannot ascertain their date. In the meantime, in consequence of the feverish state of the system, he naturally got delirium tremens. Now I need scarcely remind you that if a man of intemperate habits gets any shock of the nervous system, he is likely to get delirium tremens. Here was a case to require accurate powers of diagnosis; it might have been the delirium of fever, or of gastritis, or of bronchitis, or of drinking. You are aware that gastritis, and fever, and bronchitis will give rise to delirium, and that it may attend typhus without inflammation of the brain or engorgement of the vascular system; but in this man's case, when we connected the disease with his habits of intemperance, and looked to the history of the case, and observed that there was nothing about the head to account for his symptoms, and from his answering rationally when asked a question, we were convinced that it was delirium tremens.

You know that there are instances of delirium from bronchitis, and it is an old opinion that this arises from the blood passing to the brain in a state not sufficiently aërated, and the same thing is adduced as the cause of pain in the head. But you know that in cholera, where the blood is scarcely aërated at all, there is very little pain in the head, and the intellect remains unaffected. Some late experiments, as those of Edwards, Dr. Marshall Hall, and those which have been made in Edinburgh by Dr. Knox, seem to oppose this theory of the noxious influence of blood not properly aërated. I think that it arises rather from engorgement, as in such cases the face is generally congested and the lips purple, and that this affection originates rather in congestion than in a venous state of the blood sent to the brain. The reason which induces me to speak of this influence of venous blood is because there are certain cases of paralysis from the action of cold on the lower extremities, which may produce a permanent asphyxia of the parts affected. I knew a man whose fingers remained of a blue colour for five months, except when he put them into warm water.

To return to the case of William Fox. With respect to the herpetic eruption, it is not necessary that I should say much, except that you will most commonly find it combined with a feverish state of the system, which is said to be produced by cold. I shall also pass over his other diseases, and proceed to a more important point—the mode of treatment to be pursued. Here we had a number of co-existing diseases, varying in their seat and character, presenting a complexity of indications, and requiring a nice adaptation of remedial means. Fortunately everything but the delirium tremens depended on inflammation: they were all inflammatory diseases. This gave us an opportunity of employing the antiphlogistic plan of treatment, and we adopted it. Tartar emetic could not be given, in consequence of the state of his stomach and bowels; and its utility, so far as hepatitis was concerned, was extremely doubtful. It might have been prescribed for the delirium tremens with some prospect of advantage, for the delirium tremens here was accompanied by a degree of vascular excitement, for which bleeding cannot be safely employed without depressing the system; and opium is contra-indicated from its tendency to increase congestion: and, therefore, as the safest means of combating the disease, you have recourse to tartar emetic.

You begin with the tartar emetic; you then add a little opium, and thus go on gradually increasing the latter until you cease to give the former, and use opium alone. Opium, if given in the beginning, will increase the congestion and bring on subarachnoid effusion. I treated a case of delirium tremens in this way too boldly, and the man died with subarachnoid effusion; it was a lesson to me, and I would advise you to profit by my experience. Where you have congestion with this delirium, bleed or leech; and if you are doubtful of the issue of blood-letting, or convinced that it is dangerous, give tartar emetic with or without opium, according to circumstances. In the present instance there were other affections, namely, the pneumonia and bronchitis, which called for the use of the lancet. We bled this man, therefore, as far as his strength would allow, and applied leeches to the epigastrium. He then got calomel in large doses without opium, in such a manner as to bring him rapidly under its influence. The manner in which I prescribed it is that which is practised by most physicians and surgeons in the East Indies. I put about a scruple of calomel on the tongue, and let the patient swallow it without any liquid, or wash it down with a little cold water.

The next case I shall call your attention to is that of a man named Reddy, aged 27; he was a workman in the porter brewery of the Messrs. Guinness, and was in the habit of consuming daily large quantities of their famous XX porter, besides whisky. Three weeks before admission he was attacked with rheumatism in all the large joints, which, when we saw him, were swollen, red, and painful; the fingers of both hands were semiflexed, and he could not bear them to be touched; his countenance was dejected, and expressive of intense suffering; pulse 72, weak but regular; heart's action normal; profuse sweating; inability to move in bed; insomnia; loss of appetite and thirst. He was bled and put on the use of calomel and opium; the quantity of opium taken daily was *four* grains.

The next day, twenty-first, pericarditis was detected. There was nothing remarkable in the signs; the mercury and opium were continued; cupping over the heart followed by blisters directed, and on the twenty-fifth salivation set in; the cardiac symptoms subsided, and the inflammation of the joints greatly disappeared. The quantity of calomel was diminished from twelve grains daily, combined with four grains of opium, to three of the former with one-fourth of the latter every second day. On the twenty-sixth the rheumatism appeared much relieved, and the pulse was 88, soft, and regular, yet there was something unusual about his appearance; his countenance was excited and his eyes bright, and on inquiry we ascertained that he had slept none during the night, and that he had raved the whole time, occasionally shouting and singing. On the twenty-seventh he was much worse, he lay quite prostrated on the bed, the upper part of the body was covered with a profuse perspiration, he had twitching of all the muscles of the face, subsultus, and tremor of lower limbs; he slept none, but raved all night, and about 3 o'clock a.m. got out of bed, and endeavoured to break through a door into the adjoining ward. His tongue was dry and unsteady when protruded; he answered questions, however, rationally, and said he had *no headache;* pulse 116, very weak.

He was now ordered one grain of opium in the form of a pill every fourth hour, and four ounces of wine in the day.

On the twenty-eighth the report states that he fell asleep after the third pill (about 11 o'clock), and did not waken for six or seven hours, when he again commenced shouting and singing, but soon became quiet, and at 8 o'clock the following day the tremors had greatly diminished; his countenance was vastly improved, skin cool, tongue steady when protruded, but dry and furred, and his intellect restored. It was found necessary to increase the wine from four to sixteen ounces since the twenty-sixth.

On the twenty-eighth all the symptoms of delirium tremens had vanished; he was free from headache, his skin cool, tongue moist, and no thirst, and the pains in the joints nearly gone.

The wine and opium were now diminished gradually, and in ten days after he was discharged perfectly cured.

The complication of delirium tremens with acute rheumatism is not by any means common; and it is remarkable that in this case the first symptoms of the affection manifested themselves the day after the quantity of opium was diminished. Can we explain this by supposing that the opium acted as a stimulant, and that being stopped suddenly, it produced the same train of symptoms that usually follow the leaving off of any strong stimulant that had previously been largely indulged in ?

The explanation may seem at first plausible, but we know from experience that when opium acts *beneficially* as a remedial agent, it seldom produces any of the bad consequences that follow its exhibition in a healthy state of the body, an illustration of which this case affords : for we find that it neither occasioned headache, heat of skin, furred tongue, thirst, contracted pupil, nor acceleration of the pulse. We must, therefore, look upon the circumstance as a mere coincidence, and we can easily comprehend how delirium tremens might occur in a patient of intemperate habits during the course of a painful illness, by which he was much reduced and worn down.

Let me call your attention to some points connected with the treatment of chorea. In general chorea is a disease yielding to treatment with sufficient ease, but examples occur now and then requiring great assiduity and patience, and some which even baffle all attempts at cure. The best treatise I know on the subject is contained in the article Chorea, in Copland's *Dictionary of Practical Medicine*.

The following case was seen by Mr. Mulock, Sir Philip Crampton, Sir Henry Marsh, and myself, and exhibits in a striking point of view the difficulties the physician has to contend with in the treatment of the aggravated form of chorea, as well as the inefficiency of some of the best reputed medicines and the striking utility of others. The young lady was attacked on the 17th April with the first symptoms of chorea, affecting one side of the body only. In the course of twenty-four hours the peculiar motions of chorea had extended to all her limbs, and became hourly worse. For the first few days of her illness she could walk, although unsteadily, but she soon lost this power altogether, so strong and uncontrollable did the involuntary motions of her legs become. At the same time, she became incapable of raising her arms and hands, as they were perpetually jerking about in every direction. Indeed the rapidity with which the disease progressed was remarkable, for in the course of a week from its first beginning it had assumed a degree of intensity and violence which had no parallel in the experience either of Sir Philip Crampton, Mr. Colles, or Sir Henry Marsh.

When at its height the disease presented a truly appalling spectacle; every part of the system of voluntary muscles seemed to be affected; all the directing influence of volition had ceased, and the muscles everywhere were agitated by sudden, violent, and jerking motions, which constantly and forcibly changed the position of her limbs, throwing her into attitudes the most varied, and succeeding each other with extraordinary rapidity. Her arms were indeed thrown about with such force, that it became necessary to cover with blankets and soft padding the sides of the sofa on which she lay, and in spite of this and other precautions her limbs were soon covered with bruises. Her state was truly pitiable; one or two persons were constantly engaged in preventing her from rolling off her couch; now and then she sat up suddenly, made an involuntary effort to assume the erect position, and as suddenly flung herself down; meanwhile her limbs were flexed, extended, thrown backwards and forwards with unceasing rapidity. At one moment her hand would be struck against her head, and at the next be passed behind the back. It was almost impossible to keep her covered with clothes, for the constant motion of the limbs often tossed the sheets, blankets, and quilts off together, and not unfrequently even stripped her of her stockings.

At the height of her illness the motions of her limbs and body were quite extraordinary, and appeared to be such as could be only performed by a person whose very bones were pliant and flexible. She soon lost all power of articulation, and during a period of three weeks she was not able to put out her tongue, or speak a single word. The muscles of deglutition became engaged in the disease, but the muscular system of respiration, circulation, and digestion was unaffected throughout the disease; hence her breathing and pulse were natural, and her digestion and alvine evacuations regular. A continuance of muscular exertion, so violent although involuntary, could not fail rapidly to exhaust the system, and accordingly she lost her flesh daily, and before the middle of May, that is in four weeks, her emaciation had become extreme. Her countenance was sunk, her pulse weak, the whole surface of the body was excoriated from the friction unavoidably produced by the constant movement of both trunk and extremities.

This rendered all attempts to act on the disease through the medium of the skin quite hopeless. Leeches, plasters, blisters, liniments could not be applied; it was even impossible to administer a lavement. During sleep, and during sleep only, had she respite from the muscular labours; then she lay quiet. The liquor of the muriate of morphia proved very serviceable indeed in procuring sleep, and did not appear to produce headache, constipation, or any other inconvenience. I should, however, remark that her intellect was unaffected, and her head quite free from pain except for a few days previously to the occurrence of epistaxis in the beginning of the attack. Her appetite continued good throughout. The following brief sketch of this case has been given me by Mr. Mulock, of Charlemont Street:—

"S. W., aged 15, was affected with influenza in the beginning of April, and relieved in a few days; she continued well until the 17th, when she had a slight hysteric illness, with tossing of the left hand and arm. Dr. Graves saw her on the morning of the 18th; the disease was then manifestly an attack of chorea; the menses had appeared about two months previously, but not afterwards: Dr. Graves ordered aloetic pills combined with calomel at night, and a brisk saline aperient in the morning. She appeared to amend for a few days in her general health, but the tossing of the limbs, &c., increased; he then directed aloetic mixture with iron; I should mention that the pulse was natural, and tongue not loaded. After taking these draughts for two days the countenance flushed, and she had a slight hemorrhage from the nose; Dr. Graves left off the draughts, and ordered vegetable jellies, without either meat or wine.

"Sir Philip Crampton saw her at this time in consultation; he said the only case he had latterly was relieved by oil of turpentine, given in decoction of aloes: she took two of these draughts, but they produced so much excitement, we were obliged to give them up; the tongue became also swollen, and there was great difficulty in swallowing, indeed a person was obliged to eat before her to enable her by imitation to do so. Both sides were now affected; liquids passed out of the side of the mouth; it was impossible to give medicine either by enema or in pills. Dr. Colles was called in consultation; he ordered carbonate of iron and rhubarb in the electuary: it could not be taken, though often tried. Dr. Graves then considered that medicine ordered in the form of a lozenge could be swallowed; he thought the liquor arsenicalis in that form would be useful; this was tried for two days and appeared to be of service, with twenty-five drops of the solution of muriate of morphia, and four drops of the oil of peppermint on sugar at night; the only time jactitation of the limbs, &c., stopped was when sleep was procured.

"The prescription for the lozenge was:—

℞. Liquoris Arsenicalis, gtts. xviij.
Pulveris Gummi Arabici, ʒss.
Sacchari albi, gr. xxv.
Misce et fiat secundum artem massa.
Divide in partes sex æquales; sumat unam ter in die.

"The disease now appeared to be hysteria combined with chorea, as there were constant sobbing, heaving of the chest, and other hysteric symptoms, along with incessant tossing of the head, limbs, &c., and twisting of the eyes and mouth. She continued the liquor arsenicalis with muriate of morphia for three days; she had some rest, but when not under the influence of the morphia the disease appeared unaltered. Sir Henry Marsh saw her in consultation on the 16th of May, and ordered quina with extract of stramonium, and tepid salt-water shower baths three times a day; these were obliged to be given while lying on a hair mattress: and to continue the anodyne at night. After taking 1⅓ grains of stramonium, dilatation of the pupils took place, and it was thought prudent to leave off the medicine for some hours; the tossing of the limbs, &c., and difficulty of swallowing gradually abated.

"The form for the stramonium lozenges was:—

R. Sulphatis Quinæ, gr. viij.
Extracti Stramonii, gr. i¼.
Pulveris Glycyrrhizæ, gr. xv.
Theriacæ quantum sufficit.
Fiat massa, et divide in partes quatuor. Sumat unam quater in die.

"The entire skin, previous to using the baths on the shoulders, sides, and cheeks, &c., was in such a state of irritation from the constant friction as to require to be constantly washed with Eau de Cologne.

"19th.—The uneasiness was much lessened; the bath was of much service; her diet from the 14th was generous, as emaciation was extreme from the trifling sleep and constant motion; she has now taken the lozenges for eight days, and continues to improve; she can also take the bath sitting in an oval tub, which has been lined with wool and covered over with coarse cloth to prevent her hurting herself: the stramonium after the second day did not appear to affect the pupils."

In this case the failure of all remedies until we tried the shower bath and the combination of sulphate of quina and extract of stramonium, recommended by Sir Henry Marsh, was not more remarkable than the rapid improvement which took place after the new plan had been adopted; indeed, at the time I speak of, I considered her case as nearly hopeless, and believed that a few days would close the scene of her sufferings.

The shower bath was used at first warm, and then tepid. Its application was very difficult; the plan pursued was to place the patient on a large mattress covered with a blanket, where she was held by an assistant destined unavoidably to enjoy the bath along with her: other servants, mounted on chairs, then poured the water from several large watering pots, held high, on the patient beneath; when this was done, she was taken into another room, well dried, and then covered.

This operation, however troublesome, was perseveringly repeated three times daily; as she improved, the application of the shower bath was attended with less flooding of the apartment, as she could then be placed in a large stuffed tub to receive the affusion. From a careful observation of the effect of the remedies, I am inclined to attribute the improvement more to the shower bath than to the sulphate of quina or stramonium, although the effects of the latter on the system must have been powerful, for in a few hours after commencing its use her pupils were dilated to a maximum. Be this as it may, this combination of remedies produced a change the most astonishing, and she regained flesh, colour, strength, and command of her muscles so rapidly, that now but a slight vestige of the complaint remains.

The powerful effects of water, whether hot, tepid, or cold, poured on the naked skin, may be illustrated by many facts; but it is not easy to determine whether those effects are owing to the impression made on the sensation of the cutaneous nerves by the temperature of the fluid, or to the force with which it is applied to the surface. Both probably concur in making affusion of water so effectual a remedy: by means of cold affusion, hysterical fits and convulsive disease are frequently checked, and persons narcotized by opium or prussic acid are most speedily awakened. Water applied to the surface, whether in a continued and forcible stream, as a *douche*, or in the usual manner by means of the shower bath, frequently produces much benefit in diseases, general and local, acute and chronic. The case I have just related affords an additional example of the beneficial employment of this remedy.

Since this case occurred, I was induced by the reported beneficial effects of sulphate of zinc in certain spasmodic diseases, whether of an hysterical or of a truly epileptic nature, to try its efficacy in chorea, and I can assert with confidence that no other single remedy is so generally useful. In several severe cases it has, without the aid of any other medicine, cured the patient speedily and perfectly. In one case, which I saw with Mr. Barker, it failed altogether, and so did everything we tried except opium; which, however, was only useful in so far that it procured sleep at night, without which the patient, a boy of thirteen, must have been speedily worn out, so violent and continued were the spasmodic motions of the affected limbs. In the case referred to, time gradually brought about recovery. The sulphate of zinc may be given simply dissolved in rose-water, in half-grain doses, repeated often in the day. When *tolerance* of the salt on the part of the stomach is obtained, it will be often borne to the amount of ten or fifteen grains in the day; but we must also study its effects, and use the smallest quantity that will ensure a cure.

Authors who have written on the subject of chorea agree in stating that it very seldom persists after puberty. "We see little of it," says Dr. Blackmore, "in adults, yet it will sometimes *continue* for the whole life." It appears plain, from this observation, that Dr. Blackmore had never witnessed the first access of chorea at an advanced age, and consequently I think it right to mention that Dr. Ireland consulted me formerly respecting the late Mr. Dyas, a respectable apothecary residing in Castle Street, who, when seventy years old, was attacked by chorea in as complicated a form as I ever saw. The disease was very severe, and lasted many months. Thus do diseases of the nervous system, like the waning intellect, affect a second childhood!

The following, another instance of chorea occurring for the first time in advanced life, was communicated to me by Dr. Patton, of Tandragee:—

"The patient is a woman aged 50; four years since, her husband left her, and two or three of her children removed from this country to Scotland. She became much depressed in spirits, and *fretted* a good deal; she then had startings in her sleep and annoyance from flatulence, then the chorea came on at the end of a year, and continued, at first being severe, but with intermissions, during which she felt in better spirits. The approach of the attack was ushered in after each intermission, by lowness of spirits and sighing; the motions during the night and startings in sleep were not severe, but never left her completely. The disease has now (July, 1847) continued three years; the violent attacks never occur at present, but slight ones, which are always aggravated when the *moon is changing*, or when she has suffered fatigue or anxiety. She has never been epileptic or had a convulsion, is active for her time of life, and has a very healthy aspect. The catamenia ceased ten years since."

I have just now mentioned the good effects of sulphate of zinc in convulsive diseases; the following case is a good example of the benefit it sometimes produces in epilepsy:—T. A., æt. 39, unmarried, of full habit, liable for years to bilious attacks, and suffering occasionally from hemorrhoids, which bleed at times—after labouring for several months under dyspepsia—about the end of September, 1843, being more than usually fatigued by continued mental and bodily exertion, was seized with a fit, in which he fell from his chair insensible, but was after a little while able to get up and go to his bed unassisted. He was again seized about the end of November in the morning whilst dressing, the fit continuing from about five to eight minutes, not preceded by any unpleasant feelings, nor followed by any bad consequences. About the middle of December, same year, he was seized with another fit of the same kind, but of longer duration and severer

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character, in which the tongue was injured by the teeth; this fit was followed by great muscular soreness and lassitude, much depression of spirits, nervous anxiety, irritability of temper, and disinclination to pursue usual avocations, from the mind becoming easily fatigued and confused. In this paroxysm, as in two or three succeeding, there was frothing from the mouth, stertorous breathing, with rigidity of the muscular system, followed towards the termination by occasional twitchings of right hand and arm. In none was there from the first to the present any inclination to sleep towards the termination of the fit or afterwards, and in some time, on taxing the memory, every occurrence up to the moment of seizure, and from its termination, could be distinctly remembered. In general, the fits are immediately preceded by foolish, unconnected ideas, some muttering, a sense of suffocation, and sometimes a scream, and in some, but not all, there is seminal emission.

There was no treatment adopted till this period, when, in the last attack, from twelve to sixteen ounces of blood were taken from the arm. On consulting a medical man some time early in February, 1844, he was ordered gamboge pill with calomel, occasionally followed by black draught, five grains of assafœtida twice daily, to use the shower-bath, and to relieve the mind as much as possible from business. After continuing this plan, which had the effect of regulating the bowels and lessening in some degree nervous irritability, without otherwise causing any great change in symptoms, until June, 1844—the attacks becoming if anything more frequent but less severe—he left home to try what change of air and variety would do; this he found of use, as the fits became less frequent as well as less severe.

At this time, while in London some time in the end of July, 1844, he was advised to be cupped occasionally from the nape of the neck if any fulness of head was experienced; which was done four times, to about six or eight ounces each operation: to take four ounces of infusion of quassia twice daily, to act on the bowels when necessary, with the same pill as before used, to take as much walking exercise as possible, to leave off eating vegetables, to live on animal food and bread, and not to take more than two glasses of wine or half a glass of brandy and water at and after dinner. He followed this plan till September, 1844;

it had the effect of giving tone to the stomach and bowels; the nervous system gradually gaining strength, with a longer intermission than any before.

But he was again seized with a severe fit in September, 1844, after which he was ordered to take sulphate of zinc made into pill with extract of gentian, and to increase the quantity as much as possible; to continue the exercise so as to reduce corpulency, and after a time to alternate the zinc with the sulphate of quina, regulating the dose of it as of the former. He began the zinc in September, 1844, in three-grain doses three times a-day, and increased it to ten; at the end of twelve weeks, during which time it was regularly taken, he left off its use and began the quina, taking it in much the same doses, and alternating them occasionally, but taking the sulphate of zinc as the principal remedy till March, 1845.

This treatment had the effect of prolonging the next intermission till about the middle of November, at which time he had a slight attack. About that time the spasmodic action of the hand and arm (before described as occurring in the fit) now appeared during the intermissions at irregular times, often twice or thrice in the day, at others not for days together, and increased so much that it was not under the control of the will, being often obliged to grasp anything within reach; it also affected the right leg, not so powerfully however, and preceded by an unpleasant kind of shock, felt for the first time, passing through the whole system, and continuing severe till about July, 1845, from which time it began to lessen in severity and frequency, but recurs occasionally to the present. From about the second or third attack of the disease to the present, there is at uncertain periods, often twice, sometimes thrice a-day, again not perhaps for two or three days, a kind of oblivious state experienced for a minute or so, then going off without any particular results: this continues up to the present.

From the last attack, which occurred in November, 1844, he had a longer interval than any since the commencement of the disease; but after using zinc and quina, as I before stated, for about six months, he got tired of them, and left off their use. They had the effect of prolonging the intervals, and in some measure lessening the fits. For some time before the zinc was left off there was felt a kind of metallic state of mouth, evidently indicating that the system was saturated with the remedy. The paroxysms have continued from the date of the last, reported in November, 1844, to the present (the last occurring 27th January, 1846) at uncertain intervals, ranging from three to five, eight, and sometimes twelve or fifteen weeks, and with varied degrees of severity.

The following are the dates of the attacks in this case, extending over a period of three years; from them you will see the effects of the sulphate of zinc in prolonging the intervals between the fits:—1843, September 26th, November 25th, December 18th; 1844, February 5th, until September 3rd, when he commenced to take the zinc, he had five fits; and from this time until February, 1846, a period of eighteen months, he had but nine fits, or one every second month, while previously he had one every month.

In another case in which I was consulted by Dr. Taylor, of Bailieborough, I gave the sulphate of zinc in much larger doses, but without any evident advantage. I mention it to you chiefly as showing the quantity of this medicine that may be given without producing any injurious effects. According to Dr. Taylor's report, I recommended him on August 5th, 1845, to begin the treatment of the case, that of a young lady, by administering "one grain of the sulphate of zinc four times a-day for three days, then one grain to be added to each dose at the end of every three days, until eight grains were taken at a dose four times a-day. This course having been strictly attended to, she was able at the end of the month to take eight grains four times in the day. During this month she had four fits. About this time she complained of sickness of stomach immediately after taking the medicine, but by omitting one dose daily for one or two days she was able to take the full quantity. From the 8th of September to the 22nd of October she was occasionally able to take forty grains a-day, and had eleven fits, the majority of them of more than ordinary severity. Previous to this time I wrote to Dr. Graves on the subject, and by his advice continued the medicine at the same rate until about the 23rd of November, when I received a letter from him in which he stated that he thought it would be injudicious to persevere longer in the use of the medicine, and that he would advise the quantity to be diminished two grains daily for a fortnight, when she should be put on the use of valerian, camphor, and aromatic spirit of ammonia, in doses sufficient to counteract the general marasmus of the system which then prevailed to a very considerable extent."

It is right that I should mention to you here that the preparations of zinc, when their use has been long continued, have been described as producing general marasmus; you should therefore be careful not to continue their administration too long; it is evident that in the case I have been now speaking of, this effect was beginning to be produced, I therefore stopped the use of the sulphate gradually.

Before concluding, I wish to lay before you the particulars of another case of epilepsy derived from the patient's own account, a gentleman of the highest talent and most accurate observation. It is an example of epilepsy depending on injury to the bones of the cranium, but not occurring for some years after the accident by which the injury was produced. The following was his statement on consulting me:—"About twenty-five years since, driving out with my servant in a gig, I suddenly lost my speech, and was conscious of it for a short time, making signs to him to drive home, and then became insensible for twenty minutes or half an hour, as I was told. After this I had repeated attacks of the same kind. Having suffered previously from intense headache and intolerance of light, I came to Dublin for advice, and consulted the most eminent physicians of the day, with little advantage, for a fortnight or three weeks, when, on going one day to meet them, I observed that a swelling and tenderness had appeared on the top of the head. This was immediately examined, and I then recollected, and told them that three or four years before I had been riding rapidly along the road, and my horse had suddenly fallen, cutting its head and not the knees, that my hat was cut and dinged, and on rising on my feet I felt dizzy and confused, from which I soon recovered. The swelling was then opened with caustic, and after some time portions of diseased bone came away. Before I came up to Dublin a seton had been tried in the neck, but with little advantage—this seems to be the commencement of my disease.

"It is at varied intervals that loss of speech and insensibility have since occurred, sometimes after years, at others three or four months, but only once or twice so soon—and then only the bewildered feel, unable to articulate or to write (which I have attempted), without the insensible state accompanying or succeeding—and in every instance relieved by the discharge of wind from the stomach; indeed, I would suggest that indigestion or gouty tendency producing flatulence may in some measure excite these attacks. Subject to uneasiness in the stomach, and anxious hurried sensation and feeling of distention, with a noise of wind passing from side to side about the region of the navel on exerting the muscles, I have felt relief from chewing a bit of ginger and swallowing it, wind immediately coming away. All these annoyances generally vanish for some time after an attack of gout, which I have had occasionally in the feet at intervals of twelve or fifteen months. Warm baths or warm water to the feet always bring on faintishness, a feverish feel, and want of rest; my appetite is perfectly good, and I walk four or five miles without any fatigue, as my general allowance of exercise; at least one mile before breakfast; the bowels at times are confined, and rest disturbed by dreams and sudden awaking, in fact 'nightmare,' and then the stomach continues uneasy till wind is expelled.

"The last attack of insensibility came on after I had gone to sleep, and I doubt whether I should have been conscious of its extent had I been alone; the following day I found one eye blackened, and a black mark or bruise on the thigh near the hip-joint. I cried out on the attack commencing, as I am informed—this was the 28th or 29th of January, and my bowels had been very irregular, with pain and uneasiness low down, griping and flatulence to a considerable extent. After this attack I felt little worse, if at all, on the following days—the tongue had been a little bitten at one side, and there was rather a stiff and cramped feel in the legs. The opening whence the bone came in the head is kept open as an issue, with a bean, and sometimes becomes inflamed and painful, and may perhaps aggravate the stomach uneasiness, but the application of lunar caustic relieves this in general. Before the headache commenced, sea-bathing and swimming agreed particularly well, but any application of cold water (except to the head) now disagrees, but I use it daily to the head and neck by sponging before dressing the issue."

LECTURE XXXVI.

PARALYSIS.

Having recently met with some very interesting and remarkable cases of impairment of the muscular functions of the lower extremities, I am anxious to offer a few observations on paraplegia, particularly while the subject is still fresh in my mind: you are aware that by paraplegia is meant that species of paralysis in which the lower extremities are affected—a paralysis frequently embracing loss of motion and loss of sensation in the lower extremities, accompanied, in many instances, with derangement of the muscular power of the bladder and rectum. Now, I wish you clearly to understand that it is not my intention to describe the symptoms, or discuss the causes, of those species of paraplegia which are well ascertained, and of which you will find satisfactory descriptions in your books; under this head may be classed all those cases which are produced by disease of the spinal marrow, its membranes, the vertebræ or their ligaments, and diseases directly affecting the great nerves which supply the lower extremities. All these matters have been sufficiently studied, and require no additional observations from me; my object is to elucidate some of the obscurer varieties of paraplegia. I have in the last lecture but two touched on this topic, but I have since met with many cases, and made inquiries which tend still further to illustrate the subject. Within this last month, I have had an opportunity of witnessing a very striking illustration of the fact, that injury affecting one branch of a nerve will be propagated by a retrograde action, so as to affect another and more distant branch. A young gentleman, distinguished for the extent of his classical and mathematical acquirements, and who had just succeeded in obtaining the senior moderatorship (analogous to the *wranglership* of the English universities) swallowed a small but angular piece of chicken-bone. It lodged low down in the œsophagus, and was not pushed, by means of a [illegible] into the stomach until after the lapse of more than an hour. Considerable inflammation of the pharynx [illegible] and [illegible] was the [illegible] on the third day of the [illegible] he got a [illegible] and [illegible], which terminated in a [illegible] and [illegible] in a well-marked inflammation of the neck of the bladder.

We also find that impressions affecting the frontal branches of the fifth nerve may, by a reflex action, operate on the retina so as to cause blindness. Here the morbid action travels from the circumference towards the centre, and is again reflected towards the circumference so as to affect a separate and distinct part. Of this I lately saw a curious and instructive example. A medical student travelling through Wales on the outside of the mail was exposed for many hours to a keen north-easterly wind blowing directly in his face. When he arrived at the end of his journey, he found that his vision was impaired, and that everything seemed as if he was looking through a gauze veil. There was no headache, no symptom of indigestion, to account for this evidently slight degree of amaurosis, and yet he was recommended to use cupping to the nape of the neck, and strong purgatives. When he consulted me, which he did in the course of a few days afterwards, I at once saw that there was something unusual in the case; and, after a careful examination, I at length elicited from him the fact of his having been exposed to the influence of the cold wind. It was now apparent that the retina suffered in consequence of an impression made on the facial branches of the fifth pair. The cure was effected not by a treatment directed to relieve cerebral congestion, but by stimulation of the skin of the face, forehead, temples, &c.

It is, however, unnecessary to multiply examples to prove the truth of the proposition, that disease may commence in one portion of the nervous extremities, and be propagated towards the centre, and hence, by a reflex action, to other and distant parts. Bearing this in mind, we can explain why it is that disease commencing in one part of the system may produce morbid action in another and distant part; and it certainly appears strange that, with so many striking examples before them, pathologists should have so long overlooked this cause when seeking to explain the nature of many forms of paralysis. If certain irritations of the nervous extremities in one part of the body are capable of giving rise to a derangement in the whole system of voluntary muscles; if a local affection may become the cause of exalting and rendering irregular the functions of every muscle in the body; then, surely, it is not difficult to conceive that a cause, local as the former, and tending not to exalt but to depress the motor function of the muscles, may likewise affect not merely the nerves and muscles of the part, but also those of the whole body, or of distant organs, giving rise to paralysis.

Now, pathologists have long recognized the fact that general muscular excitement and spasm may arise from the operation of a local irritation. A man gets a contused wound on his thumb, or one of his fingers, and some superficial nerves are injured. In the course of a few days he begins to feel a degree of stiffness about the lower jaw and muscles of the neck, accompanied by a sense of constriction about the diaphragm. This increases gradually, all the muscles are thrown into a state of fixed spasm, and he gets tetanus. Here a few trifling branches of the digital nerves are injured, the morbid action is conveyed from them along the nerves of the arm to the spinal cord and brain, and is thence by a reflex action propagated all over the body. A wound in the finger causes a morbid action in its nerves, and it has been acknowledged by pathologists, that this, by acting on the brain and spinal cord, may give rise to a general morbid action of the muscular system. This being the case, there is nothing improbable in supposing that a cause affecting any portion of the branches of the nervous tree, and which produces effects of a paralytic nature, may likewise react backwards towards the nervous centres, and thence by a reflex progress may extend its influence to distant parts of the circumference.

To give another instance: how often do we see irritation commencing in the intestinal mucous membrane propagated backwards towards the brain? Take the familiar example of intestinal worms. A child labours under worms; here the irritation of the digestive mucous surface, whether it be produced by the worms or by the indigestion which accompanies them, is propagated from the stomach and bowels to the brain, and thence reflected to the voluntary muscles, causing general convulsions.

Dr. Stokes details the following case in his lectures:—"A young woman was admitted into one of the surgical wards of the Meath Hospital, for some injury of a trivial nature. While in the hospital she got feverish symptoms, which were treated with purgatives consisting of calomel, jalap, and the *black bottle*, a remedy which deserves the name of coffin bottle, perhaps better than the pectoral mixture so liberally dealt out in our dispensaries as a cure for all cases of pulmonary disease. She was violently purged, the symptoms of fever subsided, and she was discharged. A few days afterwards her mother applied to have her re-admitted, and she was brought in again, and placed in one of the medical wards. Her state on admission was as follows:—she had fever, pain in the head, violent contractions in the fingers, and alternate contraction and extension of the wrist and fore-arm. These muscular spasms were so great, that the strongest man could scarcely control the motions of the left fore-arm. In addition to these symptoms she had slight thirst, some diarrhœa, but no abdominal tenderness. On this occasion a double plan of treatment was pursued, the therapeutic means being directed to the head, in consequence of the marked symptoms of local disease of the brain, and to the belly from the circumstance of abdominal derangement observed in this and her former illness.

"She died shortly afterwards with violent spasms of the head and fore-arm; and as she presented all the ordinary symptoms of a local inflammation of the opposite side of the brain, we naturally looked there first for the seat of the disease. After a careful examination, however, no perceptible trace of disease could be found in the substance of the brain, which appeared all throughout remarkably healthy. She had all the symptoms which, according to Serres and Foville, would indicate disease of the optic thalamus or posterior lobe of the opposite side; yet we could not find any lesion whatever of its substance, after the most careful examination. But on opening the abdomen we found evident marks of disease; *the lower third of the ileum, for the length of six or eight inches, was one unbroken sheet of recent ulcerations.*" This case, gentlemen, you will perceive just now, bears very strongly on the subject of paraplegia arising from enteritis.

Again, how often do we see convulsions brought on in the same way by cutaneous irritation! A child gets an attack of fever accompanied by general irritability and restlessness. During the course of the disease the lungs become affected, and the medical attendant applies a large blister, which is left on for several hours. Next day the symptoms of nervous irritation become more violent; the child is perfectly restless, or if it dozes for a moment, awakes screaming, and is finally attacked with general convulsions. Many other examples could be brought to support this view of the question, and prove that morbidly increased action of the whole muscular system may be excited by a cause acting merely on some insulated portion of the nervous extremities.

I think, therefore, that I am borne out by analogies, strikingly exhibited by numberless examples, in asserting that the circumference of the nervous system has been too much neglected by pathologists in their explanations of the nature and causes of paralytic affections. I have given before instances of pains commencing in particular parts of the body, and travelling back towards the spine, so as to give rise to an affection of that organ which has been too generally looked upon as the result of idiopathic disease. How often does this happen in hysteria! How often does it occur that the organ primarily engaged in hysterical cases becomes during the attacks acutely painful, and as the disease proceeds, the pain travels back towards the spine, until at length the spinal cord itself becomes affected, and we find acute pain and tenderness over some portion of its track! I am fully persuaded that many modern authors who have ascribed the phenomena of hysteria and other affections to spinal irritation, have been too hasty and indiscriminate in their explanations. In the majority of cases you will find hysteric patients complain at first, not of pain in any part of the spinal cord, but in the right side in the situation of the liver, and in the region of the heart or stomach, or in the head, or the pelvic region. At this period there is seldom any tenderness over the spinal cord; but as the disease goes on, the irritation which existed in some of the situations to which I have referred is extended to the spine, and pain and tenderness are now felt over some of the spinous processes of the vertebræ. When this has taken place, then the spinal irritation thus produced becomes itself a new cause of disease, from which as a centre the morbid influence is propagated to other organs. The profession owes much to Teale, Griffin, and other writers, who have pointed out the importance of attending to this spinal tenderness in cases of hysteria, &c. Still, however, like all those who have been employed in investigating a new subject, they have, perhaps, generalized too hastily, and have in many cases regarded this spinal tenderness as a cause, when it should have been merely considered as a consequence.

Having now endeavoured to explain some of the general principles which should guide us in the investigation of nervous diseases, I shall relate some cases of paraplegia, which, though differing in their origin as to the organ inflamed, will strike you as exhibiting a close analogy to those published by Mr. Stanley.

In November, 1832, I attended with Mr. Kirby and Mr. Cusack a young gentleman aged fourteen, who was residing at a boarding school in the vicinity of Dublin, and whose case I before cursorily referred to. He had eaten a large quantity of nuts on the eve of Allhallows, and had in consequence obstruction of the bowels, attended with sense of weight and pain of the stomach, nausea, loss of appetite, and obstinate constipation. Active purgatives of different kinds were employed without effect, and the obstruction was only removed by the use of repeated enemata, thrown up with Read's syringe, introduced as far into the cavity of the intestine as the circumstances of the case permitted. To these means, assisted by leeching and stuping, the constipation yielded: but its removal was followed by symptoms of enteric inflammation, embracing not one but all the coats of the intestine—the mucous, the muscular, and certainly the peritoneal. The occurrence of a new and violent disease greatly impeded his cure; we had a long and anxious attendance, and the young gentleman escaped with great difficulty. However, the enteric symptoms at last gave way, convalescence became manifestly established, the patient was able to sit up in his bed, and as his strength and appetite were rapidly returning, he was informed that he might get up. On attempting to leave his bed, it was found that he had lost the power of using his lower extremities—in fact, he had become paraplegic. He had perfect power over his arms and trunk, but the lower extremities were quite useless. The paralysis, however, was entirely limited to the muscles; there was no diminution of sensibility in the limbs; no numbness, pain, or sensation of formication; and the muscular functions of the bladder and rectum were apparently uninjured.

Before I enter on this explanation of the case, permit me to recite the following :—I was called to visit a lady residing in the neighbourhood of Merrion Square who was said to be labouring under symptoms of dyspepsia. She had a sense of weight about the stomach, nausea, tendency to vomit, epigastric and hypochondriac tenderness—the latter situated on the right side, but no fever nor excitement of the circulation. In the course of two or three days she became slightly jaundiced, and it was evident that the latent cause of her disease was in all probability a gastro-duodenitis, terminating in an affection of the liver. It is sufficient to say that this lady's symptoms went on, and that the diseased action gradually extended to the whole intestinal tube, liver, and peritoneum. Her bowels became tympanitic, her belly extremely tender on pressure, she got low fever, with quick pulse and great restlessness, and was saved with difficulty by the repeated application of leeches and the use of calomel, so as to affect the mouth. She became convalescent; but with the return of health it was found that she had lost the power of using her lower extremities, and she continued paraplegic for a long time.

In the case of the young gentleman already detailed, you will recollect that the paralysis was entirely limited to the muscular functions of the lower limbs, and that there was no derangement of sensation, no lesion of the muscular powers of the rectum or bladder. The same thing occurred in this case. There was in the beginning no impairment of sensibility, and the power over the rectum and bladder was uninjured. Soon afterwards, however, she complained of pains in the loins and bowels, and the muscular functions of the bladder became deranged. Indeed, the case was then unfavourable; it had resisted the ordinary remedies, and threatened to become one of confirmed paraplegia, but she began to improve in about six months, and eventually recovered completely. It is to be observed that in this lady the loss of power was much more complete than in the young gentleman before referred to; his paraplegia was by no means perfect, and yielded to the employment of stimulating frictions to the extremities, combined with a cautious use of internal stimulants and tonics. In neither of these cases was the loss of muscular power so great as to deprive the patients of the use of their legs while lying in bed. They could then be raised, flexed, and nized by medical practitioners, but only in a limited point of view; for, though they were of opinion that disease of the kidneys and a discharge of ammoniacal urine were the results of spinal disease, they never seem to have reflected that the reverse of this might happen. It seems, however, now to be almost completely established, that disease of the kidneys may produce symptoms which are referable to disease of the spine. Medical men have been too much in the habit of looking at this matter only in one point of view. They know that disease of the spine will produce disease of the kidneys, and here they stop; but it has been shown that the reverse of this may happen, and that renal disease may produce very remarkable lesions in the functions of the spine. Of this very curious occurrence we have many analogies in pathology. Thus, for instance, in several cases of cerebral disease, but particularly in hydrocephalus, we have vomiting; here we have functional disease of the stomach depending on disease of the brain. Take the reverse of this,—observe the delirium which attends a case of gastro-enteritis; here you have the functions of the brain deranged in a most remarkable manner, and this produced by sympathy with an inflamed mucous membrane. The truth is, that in the spine and kidney, as well as in various parts of the body, we may have two organs so closely connected in sympathy, that disease of the one will bring on serious functional lesion of the other."

It will be seen that these observations coincide in many points with the principles I have laid down in my lecture on the subject of nervous pathology. On this point Mr. Stanley makes the following remarks:—"In reflecting on the phenomena of the first series of cases which have been detailed in this paper, it might be thought improbable that irritation, commencing in the kidney or in the bladder, should be propagated through sentient nerves to the spinal cord, and that the impression should thence be transmitted through both the motive and sentient spinal nerves to the limbs—here occasioning an impairment both of sensation and the power of motion. Some illustration of this subject seems to be furnished by the researches of experimental physiology. If in an animal, 'a few seconds after it has been deprived of life, the spinal cord be then divided in the middle of the neck, and again in the middle of the back, upon irritating a sentient organ connected with either isolated segment, muscular [illegible] * [illegible] difficult to determine whether the source of irritation be in the affected nerves, or in the central portion of the nervous system whence they are derived."

You will perceive that this explanation is, as far as it goes, though not in the same words, in meaning the same as that which I have given with this exception—that it is only an example of the general principle which I had laid down in my lectures on the pathology of the nervous system. Long before the publication of Mr. Stanley's paper, I had established the proposition that impressions made upon any portion of the nervous extremities may be propagated towards their centres, and thence by a reflex action transmitted to the nerves of other and distant parts, so as to give rise to morbid phenomena analogous to those which are produced by disease originating in the central parts themselves. Applying this principle to the subject of paraplegia, we shall find that, independently of cerebral or spinal disease, it may arise from a variety of causes, each referable to lesions commencing in distinct and isolated portions of the nervous extremities.

Thus, in Mr. Stanley's cases, the exciting cause seems to have originated in the urinary system: in the case which I have detailed, where it supervened on inflammation of the bowels, it commenced in the digestive—and it appears from a com-

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* *Outlines of Human Physiology*, by H. Mayo.

munication made to Mr. Stanley by Mr. Hunt, of Dartmouth, that the same thing may result from irritation existing in the uterine—system. Mr. Hunt alludes to several cases of disease of the uterus being followed by such loss of power in the lower limbs, that the patients were entirely confined to bed; adding that there was no change of structure in the parts to which the symptoms referred, as the source of irritation. In addition to these, I shall in my next lecture bring forward several cases to prove that a similar loss of power may be produced by the action of cold on the lower extremities. Indeed, the number of cases which I have recently met with, where paraplegia was evidently brought on by exposing the lower extremities to cold and wet, has very strongly directed my attention to this form of the disease; and I trust I shall be able, at our meeting, to communicate some very interesting matter on the subject.

after eight or nine days his back and lower extremities became affected with pain and weakness, which increased to such a degree that he was obliged to give up his occupation on the thirteenth day. He states that, during the time his back and legs were getting weak, he was obliged to pass water about three times in an hour, which he did with pain and tenesmus. On the 1st of January the pain of his back was very severe, and he lost the use of his limbs, but not completely, for he could support himself, and even walked a little with the aid of two sticks.

"At the time of his admission he appeared somewhat broken down in his general health; he was pale, emaciated, and laboured under derangement of his digestive organs. He suffered from occasional chills, succeeded by heats and sweating, which occurred at irregular periods; he also laboured under incontinence of urine and dysuria, and the stream of urine was much diminished: weakness and loss of power in his lower extremities, as reported.

"His treatment was as follows:—First, cupping over the loins, then moxæ in the same situation; attention to his digestive organs; diluents and opiates for the urethral symptoms. On the 26th of the same month, a very close stricture was found to exist in the membranous portion of the urethra. A small catgut bougie of double length was introduced, so that one half of it projected from the meatus; over this was slided a small gum-elastic catheter of ordinary length, and open at each end, until it traversed the stricture and reached the bladder; the catgut bougie was then withdrawn, and the gum-elastic catheter secured. A little constitutional disturbance followed, but soon subsided, and in a few days gum-elastic catheters of a much increased size were introduced with facility.

"*A very remarkable amendment took place in his back and lower extremities, in a very few days after the first introduction of the instrument; in fact, it was almost sudden.* Warm baths, friction to his limbs, &c., completed his cure. He was discharged on the 25th of February, at which time the power of his lower limbs was perfectly restored, and the symptoms affecting the urinary system had disappeared."

You at once perceive the extreme importance of this case; it bears directly on the question I was speaking of, and proves that urethral irritation may, as well as inflammation of the kidneys, In reference to this point of practice, I have been in the habit of using the expression—in order to fix the attention of my pupils—that such a patient has not any pain in his head, *but he has gotten his headache in the small of his back.* Now, when headache is the prominent feature of the first stage of fever how few will omit bleeding, leeching, cupping, cold or hot applications, &c. When, on the contrary, the lumbar spinal marrow is the seat of the congestion, how generally do practitioners neglect the application of topical bleeding, and other appropriate remedies. Were such neglect of less frequent occurrence, it is probable that paraplegia after fever would not be met with so often. Some may be inclined to look for the source of the paraplegia which follows fever in the irritation of the gastro-intestinal mucous surface, propagated by a reflex progress to the spinal marrow. It is not easy to decide between these two explanations, but I confess myself more inclined to adopt the former than the latter.

I shall now proceed to lay before you some facts and cases illustrating the nature of another form of paraplegia, a form of extreme interest, from the circumstance of its being hitherto but little understood, and not mentioned by any writer I am acquainted with, as well as from the peculiar nature of its origin and the frequency of its occurrence. I have, within a comparatively short period of time, met with several instances of this affection, and have some cases of it at present under treatment.

Before I enter on this part of the subject, I may be allowed to remark that, in some cases, loss of the power of motion in a limb can evidently be traced to the operation of a cause whose action is confined altogether to the surface. Thus, in the case of a woman in Sir Patrick Dun's Hospital, erysipelas occupied the calf and inside of the right leg, and occasioned some inflammation and tenderness along the chain of lymphatics extending to the groin, where one of the inguinal glands was slightly enlarged and painful. The erysipelas yielded to the employment of local and general remedies; but, for several days, and particularly while the disease was at its acme, she was altogether destitute of any power of motion in the affected limb; she could neither bend the leg on the thigh, nor could she raise the whole limb. This affection must have been produced by a reflex action propagated from the cutaneous branches to the larger muscular nerves. It

Mr. B., aged twenty-three, was remarkably strong and healthy, though of a spare habit. He was able to take a great deal of exercise, capable of enduring much fatigue, and passionately fond of hunting, fishing, and shooting, particularly the latter; and, in pursuit of his favourite amusements, frequently exposed himself to wet feet during his excursions through bog lands, and when wading in the water. These habits, however, he laid aside after the occurrence of the first attack of his illness, which happened in 1829. He had for many years been of a costive habit, his bowels being frequently confined for a week at a time, but did not experience any sensible bad effects from this circumstance, and never took any aperient medicine.

Since the first attack in January, 1829, this state ceased, and his bowels became ever afterwards inclined to looseness, which always increased before the appearance of one of the attacks, accompanied by griping, nausea, and inclination to vomit. Each attack was generally preceded by a copious secretion of insipid watery fluid in the mouth, and then the characteristic symptoms of his disease commenced. These consisted in obstinate and protracted nausea and vomiting; he first threw up whatever happened to be on his stomach at the time, and afterwards everything he swallowed, whether solid or liquid. The matter ejected was at first acid and afterwards bitter, varying in colour from mucous to bilious, but being generally of a greenish and occasionally of a bluish tinge. The greenish fluid annoyed him much, from its extreme bitterness, and the quantity thrown up in the course of a day varied from three to four quarts of fluid. He complained also of pain, referred to the stomach or lower part of the chest, which continued throughout the attack, being most acute at its commencement; for the last year this sensation had passed into a feeling of painful constriction, which he described as a "contracted feeling of his inside," and compared it to something like the effects of a cord drawn tightly, so as to compress or strangulate his body exactly along the outline occupied by the insertions of the diaphragm. During the prevalence of the attack he had profuse perspirations, particularly towards the termination of each paroxysm.

The duration of the first attack did not exceed four or five days, after which he became quite well, and continued so for six or seven months, when his symptoms suddenly returned. He vomiting went on night and day, and he was unable to retain the mildest and most soothing substances for a moment on his stomach. Sir Philip Crampton and Dr. Ireland attended him with me, and we had recourse to everything we could think of to allay the irritability of his stomach, but in vain. After continuing to resist obstinately every form of treatment for five or six days and nights, the vomiting would suddenly cease, the gentleman would exclaim, "Now I am well," and he could then eat, with perfect impunity, substances which would prove irritating and indigestible to many stomachs. This was one of the most singular circumstances I ever witnessed. The transition from a state of deadly nausea and obstinate retching to a sharp feeling of hunger used to occur quite suddenly. One hour he was the most miserable object you could behold, racked with painful constrictions across the epigastrium, alternately bathed with cold perspiration, and rejecting everything from his stomach, the next found him eating with a voracious appetite whatever he could lay hold of, and digesting everything with apparent facility.

It may be observed that as the disease in this case proceeded, the intervals between the attacks diminished, while the paroxysms increased in duration. For the last two years they continued only for four or five days, and appeared at intervals of six or seven months; latterly they used to last for eight or ten days, and returned every third or fourth week. During the paroxysm the only thing which he took was a little cold water with some brandy and a few drops of laudanum, which remained longer on his stomach than anything else, and enabled him to enjoy a few minutes' sleep. He never complained of any headache, and his intellect was remarkably clear, and his memory good.

No trace of organic disease could be detected in the abdominal viscera, and there was not the slightest tenderness over any part of the spine. He also retained to the last a complete power over the bladder and rectum.

At length his system began to give way; long confinement to bed and the frequent recurrence of these exhausting attacks completely wore him out, and he sank the 30th September, 1833. A post-mortem examination was allowed by his friends, and we scrutinized every part of his system with the most anxious care. The brain, cerebellum, spinal cord, and their investing membranes were carefully inspected; we examined the large nervous trunks that supply the lower extremities, inspected the viscera of the thorax, and searched for evidences of disease in the stomach and intestinal tube; we could find none. There was no lesion of the brain or spinal cord, no thickening or vascularity of membranes, the large nerves exhibited their normal condition, the stomach was perfectly healthy, the intestinal canal was natural, the liver and other glandular viscera of the abdomen without any trace of appreciable derangement.

Here, then, was a case of perfect paraplegia (I say perfect, for he had lost all power of his lower extremities for more than two months before his death), which may be fairly termed functional, inasmuch as there was no lesion of any part of the nervous centres to explain the phenomena present. How, then, are we to account for them? The first symptoms were undoubtedly those of abdominal irritation, as manifested by the tendency to diarrhœa in an originally costive habit, accompanied by violent paroxysms of vomiting which recurred at distant intervals. Are we to attribute this diseased condition of the stomach and bowels, which, from the remarkable periodicity of its occurrence, was evidently functional, to irritation, congestion, or inflammation of the brain or spinal marrow? From the data we are in possession of, it appears that this question must be answered in the negative. There was no headache, heat of scâlp, throbbing of the temporal arteries, or other sign of determination to the head, of congestion or inflammation of the brain, either before or during the attacks. The patient's intellect was all throughout remarkably clear, and his memory good.

Again, if we look for the origin of the disease in the spinal cord or its investments, we can find nothing to assist in explaining the phenomena. There was no pain in any portion of the spinal cord, and at no period of his illness could we detect any tenderness over the spinous processes. The history of the case seems to prove that whatever was the cause which operated on the nerves of the stomach and intestines, it gradually extended the sphere of its morbid influence to the spinal cord, and, through it, implicated the nerves of the lower extremities. The case is in many respects highly interesting, and well worthy of the attention of the pathological inquirer. The dissection was conducted in the presence of Dr. Ireland and myself, and by

Mr. Harris. It was not made in a hurried or careless manner, each organ was carefully examined, and the process occupied at least four hours.

The next case to which I shall call your attention was in the Meath Hospital under the care of Dr. Stokes.

A robust, middle-aged man was admitted into the chronic ward of the Meath Hospital, labouring under paraplegia. He stated that he was generally employed as a boatman about the the river and port, was frequently exposed to cold and wet, particularly in his lower extremities, and that he was in the habit of drinking freely. He had enjoyed good health until about seven weeks before his admission, when he was seized with numbness of the feet and legs, which, after continuing for three or four days, was followed by tingling pains running along the course of the nerves. He then remarked that the power of his lower extremities was much diminished, and this gradually increased, so as to prevent him from walking or even standing without support. His bowels became obstinately costive, and about a month after the commencement of his attack, he perceived that his urine was discharged in smaller quantity than usual, and that he was much more frequently called on to pass it than before. He also mentioned that he had gonorrhœa about six months before, and that he had used balsam of copaiba and injections.

Some time after this he said he noticed some white matter passing with the urine, but did not pay any particular attention to it, as it gave him no inconvenience. His appetite was tolerably good, and he had no headache nor any symptom of determination of blood to the brain. He denied having received any injury of the back, and there was no tenderness over the spinous processes of the vertebræ. He had no pain in the spine, either before or since the occurrence of his illness, nor was there any symptom of inflammation of the substance or membranes of the spinal cord. When admitted, he had considerable diminution of sensation and complete loss of motion in one of the lower extremities; in the other he still retained some power. He had also retention of urine, requiring the daily use of the catheter.

The treatment was as follows:—He was placed on one of Dr. Arnott's hydrostatic beds, as there was a great tendency to stripping over the hips and sacrum, a purgative pill was administered two or three times a day to remove the costiveness, and he was ordered to be cupped over the loins. The latter was done in consequence of his complaining of some tenderness on pressure in the situation of the kidneys. His symptoms, however, went on without any improvement, and he died about a month after his admission.

On dissection, the following phenomena were observed:—The kidneys, which were first examined, appeared rather soft and of a yellowish colour, but there was no vascularity, suppuration, nor other change of structure. The ureters were somewhat distended, but presented no other trace of disease. The bladder was contracted, its muscular coat thickened, and its mucous membrane very vascular. There was no affection of the prostate. On examining the spinal cord, Dr. Stokes observed that he thought the cauda equina appeared to be slightly softened, but remarked that from its appearance he could not state that it was actually diseased. The rest of the spinal cord appeared healthy and normal; there was no vascularity, effusion, nor softening. External to the sheath of the cord there was a small, flattened, oval body, about the size of half a very small hazel-nut, and of a consistence intermediate between lymph and fat. Around this there was some slight degree of vascularity. Dr. Stokes observed that, from the small size of this body, and the peculiarity of its texture, he entertained strong doubts as to its having any influence in the production of the symptoms noticed during life. He remarked, although it might have been originally the product of inflammation, and have existed in the form of an effusion of lymph, still the circumstance of its conversion into a fatty substance proved that it must have existed for a very considerable time, and the smallness of its size, as well as the obscurity of its origin, did not by any means satisfactorily explain the occurrence of paraplegic symptoms.

The last case in connexion with this subject, which I have to lay before you, appears to be analogous in its mode of origin to the former:—A gentleman of strong constitution, and extremely fond of field sports, particularly fishing and shooting, exposed himself repeatedly to wet feet at a time when he was labouring under the effects of a long mercurial course. Taking large quantities of blue pill, and exposing the lower extremities to wet at the same time, are circumstances which have an obvious tendency to produce disease, and it is not to be wondered if this gentleman became the victim of his want of caution. He got numbness and weakness in his legs, which he at first attributed to fatigue and over-exertion; but, as the disease went on, he became more and more powerless, and finally applied to me respecting his illness.

On examination, I found that he had no pain in the back, nor tenderness on pressure; nothing, in fact, to indicate any original affection of the spinal cord. The functions of the brain also were natural, and there was nothing about him to lead me to suspect cerebral disease. He had, however, considerable impairment of the muscular functions of the lower extremities, and could not walk without the aid of crutches, or some person to support him. In treating this case, I looked upon it as an instance of imperfect paraplegia, in which the paralysis apparently rose from impressions made upon the sentient extremities of the nerves of the legs and feet, at a time when these nerves were particularly liable to be deranged in their functions from the previous use of mercury. I therefore had recourse to remedies directly applied to the extremities of those nerves, and fortunately succeeded in restoring this gentleman to the use of his limbs. The cure, however, was not perfect, for a very notable degree of weakness still remains.

Of this form of paraplegia, I have now witnessed many instances. In most cases I was induced to think that it arose from impressions made by cold and wet on the lower extremities. It is most commonly observed in young gentlemen who are addicted to fishing and shooting, and who in pursuit of their amusements get wet feet repeatedly, from walking over boggy grounds, or wading in the water. It is also observed in labourers whose employment obliges them to stand in water for many hours together, as in draining, pump-sinking, and other similar occupations. In all cases it assumes the creeping form, and generally appears at first in one limb, and afterwards in the other. There is, however, considerable variety in the rate of its progress; in some cases the patients become almost completely paraplegic in a few weeks from the commencement of the disease; in others it will go on for months, and even years, before the power of the lower extremities is completely destroyed.

Where its progress is slow, it makes its approach in an insidious manner, and is at first scarcely noticed by the patient.

paraplegic symptoms. There is no pain of the head or of the spine, very seldom any tenderness, the patients are in the full vigour of intellect, and all the organs of sense in their normal condition. The functions of respiration and circulation are unaffected; and it was remarked in the first case which I have detailed, that there was no change in the pulse, either during the fits of vomiting or the intervals of ease. The appetite also is generally good ; but, in almost every instance I have met with, there has been remarkably obstinate constipation.

With respect to the prognosis and treatment of this form of paraplegia, I have but little to say. The prognosis is generally unfavourable, particularly where the disease has lasted for some time, and is accompanied by morbid irritation, or loss of power in the bladder or rectum. It is also bad in proportion to the slowness with which it has come on, and the absence of pain or formication of the lower extremities. With respect to treatment, I may observe that I have never seen any benefit derived from applications to the spine. The application of blisters or issues over the back or loins does not appear to be productive of the least good effect ; of the latter I can speak positively from experience. They are an enduring source of annoyance to the patient, and never produce the least amelioration of symptoms.

I am in the habit of applying my local remedies to the legs and thighs, selecting those parts in which the greatest cutaneous sensibility exists. What I generally do is to keep up a succession of blisters along the inside of the legs, and over the anterior and inner parts of the thighs. The practice of medicine furnishes many proofs of the utility of stimulant applications to the nervous branches, in case of disease affecting the larger trunks. Thus, in sciatica, a blister applied over the ham or calf of the leg, where many of the ultimate ramifications of that nerve are superficial, will frequently produce a much more decided effect than when applied over the origin of the nerve itself. Liniments of a stimulating kind, and blisters repeatedly applied, are the local means on which I chiefly rely in the treatment of this form of paraplegia. After some time I commence with the use of strychnia, and continue it until some sensible effect on the system is produced, when I omit its further use, and have recourse to the exhibition of sulphur. These are the two internal remedies from which I have derived most benefit. I have in such cases seen very good effects from a perseverance in the use of the sulphur electuary. Much also will be accomplished by the external use of sulphur, in the form of baths, and hence cases of paraplegia of this kind might be materially benefited by the internal and external use of the waters of Lucan, Harrogate, Baden, Barèges, &c. With respect to the use of mercury, it appears to be decidedly injurious. I have seen it given in three cases; in all it did much more harm than good.

LECTURE XXXVIII.

BELL'S PARALYSIS.—STAMMERING.—VARIOUS NEURALGIC AFFECTIONS.—INFANTILE CONVULSIONS.—MYELITIS.

I PURPOSE to devote this lecture, gentlemen, to the consideration of some other affections of the nervous system, of which I have not yet spoken; and first, as to the prognosis to be derived from affections of the portio dura of the seventh pair of nerves.

Sir Charles Bell and Herbert Mayo were the first who distinctly enumerated the symptoms attendant on paralysis of the portio dura, and drew the attention of medical men to the fact that this paralysis of the face, now popularly termed "Bell's paralysis," may often exist independently of cerebral disease; and, consequently, practitioners in general consider this affection as dependent upon some impression made upon the nerve itself, or its extremities, and unattended with danger. This view of the subject is, generally speaking, correct, but still it is liable to the following important exceptions: I have seen two cases of seizure, evidently apoplectic, in which the only paralysis that followed the seizure was seated in the muscles supplied by the portio dura. This paralysis yielded in both patients, in the course of ten days or a fortnight, to appropriate general treatment, with a succession of small blisters applied behind the ear, over the orbit and to the cheek. It is difficult to conceive how any cerebral affection can give rise to a paralysis limited to a part supplied by a single portion of the nervous system; but still such an occurrence occasionally takes place, not only in the part specified, but in the tongue and in the upper extremity. Nor is this isolation of the paralytic affection in such cases always decisive of a favourable termination; for usually, in the progress of time, another apoplectic seizure occurs, giving rise to general hemiplegia; the physician must therefore determine the degree of danger attending Bell's paralysis, and other insulated paralytic affections, not by the extent of the parts engaged, but by the cause which has given rise to them.

In almost all the cases of Bell's paralysis heretofore published, the cause has been local and external, and therefore this paralysis is usually considered to indicate no deep-seated or dangerous lesion. That it is not always so, however, the instances brought forward by Abercrombie and Mr. John Hamilton distinctly prove; for in both the disease arose from destruction of the portio dura, occasioned by caries of the petrous portion of the temporal bone, necessarily fatal. The following case is similar, and is peculiarly instructive, as proving that caries of the petrous portion may exist in a very chronic form combined with otorrhœa, and may not give rise to any urgent symptoms affecting the general health, until long after the portio dura has been destroyed, and Bell's paralysis been produced.

From an attentive consideration of the history of the following case, it would appear that the disease first destroyed the membrana tympani, the internal ear, the ossicula, the portio dura of the seventh pair within the aqueduct of Fallopius, together with a good deal of the petrous portion of the temporal bone on that side which looks towards the tympanum. During this stage Bell's paralysis was produced, and profuse otorrhœa existed without any cerebral disturbance. But as the disease ate its way inwards, until it perforated the dura mater, the matter formed found a readier exit into the cavity of the arachnoid, and an entirely new set of symptoms commenced, denoting cerebral and spinal disturbance. The cessation or diminution of the flow of matter from the external ear at this point of time cannot therefore be considered as the result of a vicarious suppuration set up in parts more deeply situated, but must be regarded as the simple result of the fact, that the progress of the disease had formed a new opening internally, into which the matter found a readier vent.

A boy about ten years old was admitted into the Meath Hospital, labouring under general dropsy. He appeared of a scrofulous habit, and was much worn down by long-continued diarrhœa. Under appropriate treatment his symptoms gradually but slowly disappeared, and he was restored to comparative health. We now observed that the right side of the face was affected with paralysis, and on examination found that he had been subject to a discharge from the right ear for seven years previously. The paralysed cheek presented the phenomena usually observed in "Bell's paralysis." He was attacked soon after with acute pain in the ear, and in the left side of the head; a fortnight after, convulsions set in; the pain moved from the side to the back of the head, then to the back of neck, and ultimately extended the whole way down the spine, and about this period the otorrhœa diminished. A few days before death *he was attacked with spasms resembling those of tetanus, and the surface of the body became exquisitely tender to the touch.* He never had any loss of motion, and to the last his intellect was perfect. From the period when the pain set in to that of his death, the convulsions returned about six times.

Post-mortem.—The portio dura was dissected on the face, and found healthy; the nerve was also healthy from its origin at the base of the brain to its entrance at the meatus auditorius; immediately above this opening the dura mater was of a greenish colour, detached from the bone as if by fluid, and perforated by a round hole large enough to admit a small crow quill. On dividing this part of the membrane, the space between it and the bone was occupied by a thick, greenish, and offensive pus, and the opening in the dura mater was observed to lie exactly opposite the foramen in the petrous portion of the temporal bone called the *aqueductus vestibuli;* this opening was much enlarged, and the bone around it was in a carious condition. The nerves at the base of the brain were bathed in this thick green pus, but the organ itself was everywhere healthy, and free from any excess of vascularity. The arachnoid was nowhere thickened or opaque, and the pia mater not more injected than natural; the ventricles were not distended. Our attention was next directed to the state of the spinal cord; the theca vertebralis was much distended by the same kind of matter, which flowed abundantly from any accidental puncture of the membrane. The matter was contained in the sac of the arachnoid, which membrane was quite healthy, and presented its usual glistening appearance, no thickening or opacity observable in any part of its extent; the pia mater was also free from disease; all the attachments of the *ligamentum dentatum* remained unbroken. The spinal marrow, on being slit up, presented no trace of disease; the roots of all the nerves from the base of the brain to the cauda equina were bathed in pus, the presence of which fluid on the surface of the brain and spinal marrow had no doubt irritated these organs, and occasioned the tetanic symptoms and the cutaneous tenderness.

Mr. MacDonnell, my clinical clerk, traced the portio dura through the aqueduct of Fallopius; about a quarter of an inch from its entrance, the nerve was completely divided; the petrous portion of the bone was extensively destroyed, and presented a mere shell; the membrana tympani and all the internal ear were destroyed.

The following case contrasts in an interesting way with the former, exhibiting the vitality of the parts supplied by the portio dura, affected exactly in an opposite manner; for the muscles that in the one were paralysed, were in the other subject to a spasmodic action which lasted for several months, and during the period of its greatest intensity returned about every fourth second. I am not aware that this disease had been hitherto described, and therefore am authorized to give it a name; and, accordingly, in honour of the great man to whom we owe such extensive discoveries on the physiology and pathology of the nervous system, and who has more particularly thrown such light on the affections of the portio dura, I propose calling it "*Bell's spasms of the portio dura.*"

A woman named Quinn, aged 40, of spare habit, was admitted into the Meath Hospital, June, 1841. She stated that her complaint commenced four years and a half before, in the following way:—" The lower eyelid of the right eye became affected with spasmodic twitches, producing a kind of winking; and other muscles of the face which receive branches from the portio dura, and which it is unnecessary to enumerate, became affected in a similar manner by degrees. This disease was unpreceded by pain in the head, ear, or any part of the face. Her general health was good. On admission, all the muscles of the face supplied with the seventh nerve were affected by spasmodic contractions, occurring many times during a minute. The angle of the mouth and ala nasi of the right side were pulled towards the ear; the lower eyelid closed in a peculiar manner, producing a rather ludicrous kind of winking. It was also observed that the platysma myoides participated in each spasmodic contraction, and its fibres were seen throwing themselves out strongly in relief, in well-marked bundles. She also complained that the os hyoides was sometimes pulled towards the right ear. *These phenomena occurred also during sleep*, and were greatly exaggerated by any kind of excitement. She complained of constant noise in the right ear without any pain; but the sense of hearing was quite unimpaired. No diminution of sensation, or alteration of the temperature of the affected side. Her health was good.

The phenomena presented in this case were all owing to some unknown affection of the portio dura. The only muscles engaged were those receiving branches from that nerve. We know that, on quitting the stylo-mastoid foramen, the portio dura sends a branch to the stylo-hyoid muscle and another to the digastric, both which muscles being connected with the os hyoides, will, of course, when affected by spasms, drag that bone towards the ear of the same side. In the substance of the parotid gland the nerve divides into two large branches; one ascends on the face, called the temporo-facial; the other, the cervico-facial, assists the former in supplying the muscles of the face and chin, and also sends some *remarkably long branches to the platysma myoides muscles*, and the other superficial muscles of the neck. Can we explain the constant noise in the ear, *unaccompanied by pain or loss of hearing*, by a similar spasmodic action of those small muscles of the internal ear which receive branches from the portio dura, by which a muscular *bruit* was produced, the intensity of which may have been greatly exaggĕrated by its vicinity to the organ of hearing.

Let me next call your attention to neuralgic affection of the larynx. The first case which I shall speak of occurred in a young lady originally of vigorous constitution, but latterly suffering from menstrual irregularity and hysteria. The laryngeal affection had been considered to be inflammatory in the country, and had been treated with purgatives, leeches, blisters, antimonials, and finally mercurialization. No relief had been obtained, and she came to Dublin, where she was placed under my care, and that of Sir Henry Marsh and Mr. Barker. The pain had become almost constant when we first saw her, but was by no means violent, except now and then, when it used to become suddenly aggravated. These paroxysms of pain could not, properly speaking, be called violent; they were, however, distressing, and amounted to a most annoying feeling of distress about the whole region of the larynx. There was no external tenderness, and the internal fauces were healthy. We considered it to be a hysterical nervous affection. This neuralgia was chiefly remarkable for a change of tone and weakness in the voice, which invariably attended the paroxysms, showing that the *rima glottidis* and the *chordæ vocales* were the parts chiefly implicated. We must suppose, therefore, that the pain was derived from the branches of the superior laryngeal nerve, which Dr. Reid has proved to be chiefly *sensitive.*

The alteration of voice which accompanied the paroxysms of pain must be considered as a proof that a superior laryngeal nerve has some influence on the motions of the vocal organ; unless, indeed, we adopt the supposition that the affection extended likewise to the inferior laryngeal nerve. The facts of the case contain nothing decisively confirming or negativing either hypothesis.

We first gave large doses of carbonate of iron, which had the effect of rendering the attacks periodic. Every morning, at *ten o'clock to the minute*, the paroxysm commenced. The dose of iron was now increased, afterwards sulphate of quina, and finally arsenic was employed, but without any corresponding improvement. The degree of suffering became, indeed, less severe, and its duration less protracted, but it appeared extremely doubtful whether the improvement was not owing more to time than to medicine. Under these circumstances we thought it prudent to desist from all active treatment, and we recommended change of air, scenery, and the use of chalybeate mineral waters.

This case affords a striking example of the curious fact, that medicine administered for the purpose of relieving a disease more or less fluctuating or remittent in its character, will sometimes render it strictly periodic, with marked paroxysms and free intervals. Having produced so striking an effect with our remedies, we are apt to calculate with confidence on still further improvement, and we increase the doses of tonics with boldness and full of hope; disappointment, however, here awaits us, for no tonic will be found capable of affecting any further alteration or shortening of the fit. In such cases we cannot be too much on our guard, lest we injure the constitution by too frequent attempts to procure a diminution of suffering.

Loss of speech arises sometimes from lesions of apparently a very trifling character. A person may totally lose his speech without any previous existing or premonitory symptoms indicative of nervous lesion—without having experienced any sensation of pain or vertigo, any noise in the ears, any indications of determination to the head—in fact, without anything to show that the aphonia was connected with any particular state of the brain. Thus, a barrister whom I attended with Dr. Beatty, was walking up and down the hall of the Four Courts waiting for a case to come on, and chatting with one friend and another; as the hall was rather crowded and hot, he went out into the area of the courts for the sake of the air, and had not remained there more than ten minutes, when an old friend from the country came up and spoke to him. He was pleased to see his friend, and wished to inquire about his family, when he found to his great surprise that he could not utter a single audible sound; he had completely lost his voice. He recovered the use of his tongue in about three weeks, but not completely, for some slowness of speech remained. When loss of speech was first perceived, his friend brought him home in a carriage; and during the day he had several attacks of vertigo, and afterwards hemiplegia. For several hours, however, before distortion of the face or any of the usual symptoms of paralysis had commenced, the only existing symptom was loss of speech. This gentleman died of apoplexy in about two months.

In many cases of paralysis you will find that although the patients have lost the power of utterance, yet the motions of the tongue appear to be nowise deranged. In the majority of cases it can be shortened, elongated, raised, depressed, or moved from side to side, with as much apparent facility as in a state of health; and yet the voice is in some instances very much impaired, in others totally lost. In such cases it would appear that the defect lies in the glottis, which forms and modulates the voice, and not in the tongue or lips, which divide and articulate it. Indeed this is evident to any one who observes the interrupted and spasmodic efforts which paralytic persons make when speaking; they are in fact all stutterers.

A young gentleman of delicate constitution, and who is now about sixteen years of age, continued to enjoy tolerably good health up to his sixth year. When about six years of age he went to bed one night in health, and without any unusual symptom; but on getting up in the morning it was observed that he had lost his speech, and was unable to articulate a single word. His family became alarmed, and sent for a physician immediately; the boy got some internal medicine and a stimulant gargle, and recovered his speech in a few days, without the occurrence of any symptom of laryngeal inflammation or cerebral disease. But what was remarkable in the case was this: the boy, who up to this period had spoken well and distinctly, now got a terrible stutter.

This resisted all kinds of treatment, and for ten years he continued to stammer in the most distressing way, and was so annoyed by it himself, that when a boy he used to stamp on the ground with vexation whenever he failed in uttering what he wished to express. In the month of May last he got an attack of chronic laryngitis of a scrofulous character, and evidently the precursor of phthisis. Dr. Stokes and I have examined him, and we feel convinced that tubercular deposition is going on in the lungs. But what is most curious in the case is this: after he got the laryngitis, a very peculiar change took place; the laryngeal inflammation modified the tone of his voice so as to make it a little husky, *but the stammering has completely ceased.*

You are aware that stammering has been explained as depending on spasm of the muscles which are employed in modifying the column of air as it rushes through the narrow aperture of the glottis. At certain times, and under a variety of circumstances, those fine muscular organs become spasmodically affected, the vocal chords no longer undergo the same steady and exact tension and relaxation, and speech becomes interrupted in consequence of frequently recurring closure of the glottis.

In the case to which I have referred, inflammation taking place in the mucous membrane covering these delicate muscular fibres, you can conceive that either the thickening of the mucous membrane, or the alteration in the state of its vitality, may have so modified the disposition of the parts, that they become incapable or indisposed to undergo those rapid contractions necessary to produce stammering, by inducing closure of the glottis at the moment that its aperture ought to remain open. The case itself, however, is an extremely curious one, and I do not believe that there is any similar one on record. Everything which bears on the cure of so important a disease as stammering, even though it be accidental, and not the result of medical care and ingenuity, is of great value, inasmuch as it tends to place the causes of the disease in a clearer light. In this point of view I look upon the case as one of very great interest.

There is one curious fact with reference to stammering which I do not think has been before noticed, namely, that women very rarely stammer. In a family of my acquaintance, this defect of the speech has been hereditary among the males for three generations, but the females have in no single instance been so affected.

With respect to the cure of stammering, I have recently discovered a method by which the most inveterate stutterer may be enabled to obtain utterance for his words with tolerable fluency. It is simply by compelling him to direct his attention to some object, so as to remove it from the effort he makes to speak. Thus, I direct him to hold a rule or bit of a stick in his right hand, and with it to strike the forefinger of the left in *regular time* with the words he is uttering; the eye must be fixed, and all the attention directed to the finger he is striking, and the time must be strictly kept with the syllables. This method I have tried in several instances with complete success, and Dr. Neligan informs me that since I first mentioned it to him, he has found it completely effectual in numerous cases. Although, of course, when thus employed, this plan can only be regarded as a means of affording temporary relief, I have no doubt that if it were perseveringly followed out with young persons who stammer, both in reading and speaking, it would cure them permanently of this unpleasant affliction. Its efficacy would seem to prove that stammering is altogether a nervous affection.

With reference to neuralgia, we find that it attacks various parts of the body, and amongst others the mammæ.—An unmarried lady, residing in the neighbourhood of Dublin, consulted me in July, 1829, for this affection. She was of the sanguineous habit, robust, and otherwise healthy. The disease had lasted two years with various degrees of violence; the breasts being at times nearly free from pain, but generally they were very troublesome. During the paroxysms, which often lasted several days, and sometimes considerably longer, the mammæ, which in this lady were full and large, became extremely painful and tender, but were neither tumefied, hard, nor red. The intervals between the paroxysms were marked not only by a total cessation, but by a gradual diminution of pain. At no period had there been any spinal tenderness. One breast was not more affected than the other, and the axillary glands were not swollen. She had consulted several practitioners, had taken much medicine, and made use of many topical applications, without relief. Leeches had been repeatedly applied, but their bites had invariably caused excruciating pain, and the bleeding they occasioned was not followed by the least relief.

I at first tried stupes, narcotic liniments, and plasters, with warm salt-water baths, but these measures were unattended with the least improvement. The absence of complete intermissions, and of well-marked paroxysms, prevented me, during several weeks, from perceiving the true neuralgic nature of this pain; at last this view of the subject occurred to me. I tried the carbonate of iron with marked benefit. The disease has since frequently recurred, but its violence has always been lessened by the carbonate of iron. Sea bathing she likewise finds useful. I may here observe that in those cases of neuralgia in which carbonate of iron proves useful, I never found it necessary to raise the dose beyond one drachm three times a day. Indeed, a larger dose than half a drachm is seldom required. This statement of my experience I consider necessary to counteract the impression made on the minds of students by a perusal of some of the London periodicals, where enormous doses of carbonate of iron are recommended by Dr. Elliotson.

I have examined this subject in a practical point of view with great attention, and think that what is true concerning carbonate of iron applies also to most *tonic* medicines. In fact, we may consider it as a general rule that tonics are rarely indicated where moderate doses do not effect the desired purpose. This applies more particularly to the stronger tonics, such as the salts of iron, of arsenic, and quina. I can scarcely conceive a case possible in which a judicious physician will find it necessary, for instance, to give more than ten grains of sulphate of quina in a day, and yet much larger doses are not unusual here and elsewhere. Whenever the symptoms supposed to call for such a treatment resist moderate doses of sulphate of quina, we ought to pause, and reflect whether another plan of treatment ought not to be adopted.

There are two states of the system attended frequently with well-marked rigors, febrile paroxysms, and intermissions closely resembling ague; I mean internal suppuration, and local inflammation without suppuration. Practical physicians are fully

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Neuralgia of the testicle is not a very common form of disease, but it requires notice, as it gives rise to excruciating agony, and constitutes one of the most painful affections that can be imagined. I have seen two examples of it within the last year; the first was a young gentleman of highly irritable nerves, who had studied hard and dissipated much; in him the paroxysms of pain did not observe any very marked period, but returned daily at uncertain intervals, which grew shorter and shorter, until at last he had scarcely any respite day or night. There was no fever, and not the slightest appearance of local congestion or inflammation. When attacked with a paroxysm the patient would throw himself on the floor, and roll about in the greatest agony, covered with a cold perspiration. This case yielded to large doses of carbonate of iron freshly prepared, and frequent inunction of the testicle and cord with belladonna ointment. The second case of neuralgia of the testicle occurred in a gentleman who laboured under neuralgic pains, decidedly of a gouty nature. In him the pain of the cord and testicles used to come on every afternoon about four o'clock, and continue for several hours. The pain, though considerable, did not approach the degree of agony experienced in the first case. It was at times, however, so severe as to compel him to groan aloud. This neuralgia of the testicle disappeared after a few days, and was replaced by a violent gouty pain in the loins and right hypochondrium. The latter yielded to the usual local treatment and the use of colchicum internally.

A man was admitted into the chronic ward a few days ago, who cannot separate the lower from the upper jaw to the distance of more than two lines. What are the cases in which we find this immobility of the lower jaw? Most commonly in tetanus or locked-jaw; but here this cannot be the case, for the man has no sign indicative of a tetanic affection, no rigidity of the muscles of the neck; his countenance is very different from that of a tetanic patient, and he has not been exposed to any of the ordinary exciting causes of that disease. But, leaving all consideration of the nature of the disease out of the question, what is it that prevents him from moving his lower jaw? It must depend on one of two causes; either the muscles which perform the motions of the lower jaw are stiff, rigid, and incapable of motion, or else there is some disease of the articulation which obstructs the motion of the bone. This proposition is universally true of all articulations, that when they become impeded or completely obstructed in their motions, the derangement arises from some abnormal condition of the muscles, or of the bones and ligaments which form the joint.

In this case we find that, in addition to being unable to perform the proper motions of the lower jaw, the patient has intense pain, darting from the angle of the jaw towards the temple, the ear, and the side of the neck. This pain is of an extremely violent character, so as to resemble tic douloureux, and the resemblance is still further increased by its being more or less intermittent. Now, on inquiry into the history of this case, we find that the patient had some time ago laboured under toothache, for which he had the last molar tooth but one of the upper jaw extracted, and that immediately afterwards he was seized with violent pain in the part, and found that he could no longer move his lower jaw as usual. I have seen many cases of this kind, in which a painful or carious tooth, or an injury done to the gum or jaw, has been followed by violent darting pain in the nerves of the face, simulating in many particulars tic douloureux.

I remember being sent for to Middleton, near Cork, some time since, to see a young lady of delicate constitution, whose health was materially deranged from what was said to be an attack of tic douloureux. She had been under the care of many practitioners, and had used very large doses of the carbonate of iron and sulphate of quina, and at the time I visited her was taking arsenic. The first thing I did on my arrival was to examine her teeth. On close inspection, I observed that on the crown of one of the upper molar teeth there was a spot which appeared to be decayed, and found on inquiry that she had frequently suffered from pain in this spot when she drank any cold liquid. I had the tooth drawn, and soon afterwards the pain completely ceased. Yet in this case the pain was not only of an intense character, preventing sleep and wearing out her strength, but it had its intermissions, and was aggravated at particular hours of the day.

Another instance of the same kind came under my notice about twelve months ago. A young lady was brought to me by a medical friend of hers to have my advice for an attack of tic douloureux. She had been attended by this gentleman with great care, and no mode of relief left untried, for her sufferings were intense, and she had constant exacerbations of pain. I asked him were her teeth sound, or had she any disease of the gum or jaw? He said not, and that he was sure of this, for he had examined her teeth over and over again. On opening her mouth, however, I thought I saw some unsoundness in one of the teeth, and recommended her to go to Mr. M'Clean and get it drawn. She did so, and the pain quickly disappeared.

I could also give you many cases in which an injury done to some of the branches of the dental nerve has given rise to symptoms closely resembling those of tic douloureux. One of the most curious circumstances connected with such cases is, that the pain is always of a more or less intermittent character. The same thing is observed in that form of headache which arises from irritation of the brain, produced by spiculæ of bone growing from the internal table of the skull. In a case which occurred some time back at the Meath Hospital, where several spiculæ, some of them more than a quarter of an inch in length, were pressing on the brain, the headache was of a distinctly intermittent character. This remarkable periodicity of exacerbation, in cases where the operation of the exciting cause continues still the same, seems to be peculiar to the nervous system.

In many cases considerable derangement of the facial nerves is found to follow an injury done to some branch of the dental nerve in drawing a tooth. When the bone has been injured by the force used in extracting the tooth, it frequently happens that, if the injury be not quickly repaired, and the parts healed up, symptoms resembling those of tic douloureux or rheumatic neuralgia will supervene, and give the patient a great deal of annoyance. Such was the origin of the mischief in the case before us; the man received an injury of the upper jaw in drawing a tooth which is not as yet healed, as you may perceive by introducing a probe between the separated portions of gum, when you will find it grate against the rough surface of the bone. In addition to this, there are considerable tenderness of the gum and swelling of the neighbouring parts, which have extended to the muscles, their sheaths, and finally to the articulation of the lower jaw. You can satisfy yourselves of this by examining the parts and striking the lower jaw, so as to press it suddenly upwards and backwards into the glenoid cavity, just in the same way as you press the thigh bone against the acetabulum when you wish to ascertain whether there is inflammation of the hip joint. The motion of the lower jaw is here prevented by inflammation, extending from the upper jaw, so as to involve its ligaments and the neighbouring muscular sheaths.

There are other causes also which may be attended with the same diminution of motion in the joint. Thus, a man may get an attack of rheumatism in the scalp, which may extend to the temporal muscles, and prevent him from being able to depress his lower jaw; and I have known cases in which this condition of the temporal muscle has given rise to suspicions of the existence of trismus. When you examine the articulation you find nothing amiss, but when you come to press on the temporal muscle, above the zygoma, the patient complains of pain and tenderness. The irritation produced by rheumatic inflammation gives rise to a fixed rigid state of the muscle, and hence the patient cannot open his mouth. This form of disease I have described long since in a paper published in the Dublin Hospital Reports. It can be relieved with great ease by applying leeches to the temple, and ordering the patient to rub over the part a small portion of mercurial ointment with extract of belladonna, two or three times a day. The same state of the temporal muscle is sometimes observed as resulting from an extension of inflammation, in case of a wound of the scalp in its vicinity.

In the case before us, almost everything will depend on the process which nature may adopt with respect to the injury of the maxillary bone. If the bone throws up healthy granulations, and the inflammatory process ceases, the affection of the nerves, as well as of the muscles and joint, will quickly subside. All we can do under the circumstances is to apply leeches over the side of the face, and order the man to rub in mercurial ointment; everything, however, will depend on the turn the disease of the bone may take.

Let me next call your attention shortly to infantile convulsions, more especially those which attack children at the ages of two, four, and six months, and to the utility of oil of turpentine in their treatment.

When we consider the convulsive affections of the infantile period, we find that they may arise from a variety of causes. In the first place, they may be produced by the process of dentition. Some persons seem to think this impossible; but it is not only possible, but true: for teething is capable of exciting a very great degree of irritation in the system. We also observe that an irritable state of the brain, accompanied by a hydrocephalic tendency, will produce convulsions; but in very many instances, particularly in children of the ages mentioned above, they proceed from intestinal irritation. Of those forms which spring from the irritation of dentition, or of cerebral excitement, I do not intend to speak, as, on these matters, the standard medical works furnish abundant information. I shall restrict myself, therefore, to some observations on those convulsions which depend on intestinal irritation.

As such convulsions frequently arise from causes which affect digestion, and produce a change in the mode of nutrition, they appear very soon after birth. The animal which but a short time before was nourished by the placenta, is now supported by ingesta; and hence, from this sudden change, if there be any source of irritation existing in the system of the child, or in the nature of its food, an unhealthy state of bowels rapidly ensues. To the consequences of this affection, manifesting itself so soon after birth, nurses have given the name *nine-day convulsions*. Again, when another change is made, and the nurse's milk is left off, children are also liable to convulsive fits, and these are the convulsions of ablactation. In fact, at any period during the first year infants are very apt to get convulsions from various causes. If the mother uses an improper kind of food or drink, or gets into a bad state of health, or be strongly affected by mental emotion, the quality of the milk will be suddenly changed.* Under all these circumstances, or if the child be over-fed—a very common fault—the bowels get out of order, the whole intestinal canal is thrown into a state of irritation, and convulsive fits succeed.

It is necessary to be more explicit on this subject. When you are called to treat a case of infantile convulsions, bear in mind that they very frequently arise, particularly during the first six months, from the cause before mentioned, and this should, therefore, claim at once your attentive consideration. I remember the time when it was the common practice to treat every case of convulsions as if it were an hydrocephalic attack, and when antiphlogistics, calomel, and cutaneous irritation were the indiscriminate means employed in combating every form of this disease. If a child happened to get a convulsive fit, it was immediately said, here is inflammation or congestion of the brain; and leeches were applied in successive relays, calomel given in large doses, egg-shells, crabs' eyes, magnesia, and other absorbents administered, and the unfortunate infant cruelly tortured by the repeated application of blisters to the scalp. I have seen cases where this blistering was carried to such an extent, that the child had not a place to rest its head upon.

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* The custom adopted by some of keeping the child at the breast for a year or a year and a-half is both unnatural and injurious. Every child should be weaned when nine months old.

It is to Dr. Gooch we owe the valuable discovery that there is in children a state of heaviness of head and torpor, accompanied by a tendency to convulsions, in which depletion cannot be employed, and where narcotics and even stimulants may be used with advantage. Dr. Locock asserts that convulsions of this nature may be recognized by the depressed state of the fontanelle, an assertion which I have not verified. With respect to leeching, I have to remark that a single leech to an infant is equal to a bleeding in an adult; and yet how often have we seen children leeched and leeched, until, becoming pale and exsanguineous, they sink as much from loss of blood as from the effects of disease.

With respect to the causes and periods of indigestion in children, I have already spoken. There is one point more which I wish you to hold in memory. Milk is a compound fluid, a beautiful emulsion furnished by the hands of nature, in which sugar, oil, and curd are blended with a certain proportion of water. Now, when a compound fluid such as milk enters the stomach, and is submitted to the process of digestion, those parts which are soluble in water are absorbed, and those which are not become first coagulated, and afterwards undergo resolution in the gastric juice. Thus, while the water and sugar are absorbed, the curd of the milk is separated from it by coagulation, and forms a solid substance which is acted on by the stomach, and becomes dissolved by the agency of the gastric juice, and in this way contributes to nutrition. Not a particle of the milk, however, ought to enter the duodenum until it has passed through the usual process of digestion. As the first step to the accomplishment of this is the coagulation of the curd, this occurrence takes place with extraordinary rapidity: and it is a sign of health if the milk be thrown up in this state immediately after it has been sucked. The rennets of young animals give striking evidence of this power. But if it should happen that the stomach does not act properly, and the curd remains undissolved, what is the consequence? The curd passes into the alimentary canal in a condition different from that in which nature intended it should, and consequently produces intestinal irritation. None of the purgatives given to children are attended with half so much griping as this substance.

This explains the phenomena which in such cases present themselves to our observation. The child becomes griped, irritable, and feverish, his tongue is loaded and white, he gets restless, and now and then utters a shrill scream. In this way the disease may go on for a considerable time; as the child is dropping asleep, he starts suddenly, and screams out, bends himself in the form of an arch, and throws his head back as in opisthotonos. I have seen children in this state for a week. The physician or nurse gives castor oil or some other purgative, and a great quantity of the curds are passed, and surprise the child's relatives. On examining the discharge, you find it consisting of lumps of different sizes, covered imperfectly with bile, and having a burnt appearance; on breaking them up, you perceive them to be white internally, and consisting of indigested curd. You remove them by purgative medicine, and the child gets well.

Now, we all can do this; it is clearly laid down in books: you are told to examine the egesta, and give purging medicine where it is necessary. But there is one fact which has not been noticed. When you have treated the child in this way, and the attack has been cured, if the child is very strong when put to the breast again, he may go on well, and you have no further trouble; but if he is weakly, or of an irritable habit, when he is brought back to the suck again, or spoon-fed with milk, the same process of imperfect indigestion takes place, and he gets another fit. The physician is again called in, and repeats the purgative, and the child goes better a second time: and in this way the physician goes on giving medicine, and the mother giving milk, and everybody wonders at seeing such a quantity of [illegible] stuff passed from the bowels. How are you to avoid this? By making the infant abstain from milk in any shape for twenty-four hours, sometimes for the space of two or even three days. It is astonishing how small a portion of milk, even in the most diluted state, will keep up this disease, acting like a species of poison in the stomach, however diluted. You know that animal poisons, such as the vaccine or variolous virus, will affect the system even when applied in a state of extreme dilution, and you can therefore conceive that a small portion of milk will operate in this manner.

I attended a case of this disease some time ago: the child had a relapse, and, on being called in again, I asked the mother whether she had given it any milk, and she told me scarcely any. I am always suspicious when I hear the word scarcely used: and, on requesting to see the kind of food she had been administering, she handed me a bowl of barley-water, with the usual proportion of milk and sugar in it: it is in this way that we see the disease prolonged week after week by the prejudices of the nurse and the ignorance of the physician. Well, if you forbid milk altogether, what will you give the child? Let him take chicken-broth, barley-water, thin panado, veal-broth, or whey. How long are you to continue this? The number of days will depend on the power which the child possesses of regaining the proper tone of the stomach; some children will have the stomach out of order to-day and well to-morrow, and the length of time you are to keep up this diet will vary considerably.

When you are called, therefore, to a case of convulsions, inquire into the history of its symptoms, the nature of the alvine evacuations, and the quality and quantity of your patient's food; and, if you find that before the attack the child's bowels have been in a bad state, that they have been for some weeks inclined to be loose, or that the stools are, at the time, similar in colour and consistence to what I have described (though, by the bye, you are often told that everything is quite right when it is not the case), you will then be able to judge properly of the nature of the case, and, by giving aperient medicines, you will probably not only cure the disease, but also prevent a return of the convulsions. Sometimes, however, the convulsive fits will remain after the irritating sordes have been removed by purgative medicines. Absorbents are next made trial of. These have a very beneficial influence in many cases; they can do no harm, and where acid is present (and this occurs in the stomachs of children to a greater extent than in those of adults), prove mildly purgative.

But if the convulsions continue, what else will you prescribe? I remember attending, not long since, an infant about three or four months old, who had been for some time under treatment for convulsions. Leeches had been applied to the epigastrium; it got calomel, castor oil, and hydrargyrum cum cretâ, absorbents, aperient and fœtid enemata, and blisters to the vertex and stomach. Still the convulsions went on. Well, what did I do? I prescribed the following mixture:—

℞. Olei Terebinthinæ, ʒj.
Olei Ricini, ʒiv.
Syrupi Papaveris albi,
Mucilaginis gummi Arabici,
Aquæ fœniculi, āā, ʒij. Misce.

Of this mixture, when well shaken, exactly ʒj. was to be given every third hour, and what was the result? It operated on the bowels, and produced a copious discharge of urine, a marked improvement took place, and towards evening the convulsions entirely ceased.

Dr. Brereton informs me that he has, in similar cases, after the bowels were evacuated, succeeded in preventing a recurrence of the convulsions by means of the following mixture, suited to a child six months old:—

℞. Olei Anisi, gtts. iv.
Sacchari Albi, gr. x.
Intime misceantur et adde,
Aquæ, ℥ij.
Pulveris Rhei, gr. x.
Carbonatis Magnesiæ, ℈j.
Tincturæ Opii, gtts. iv.
Spiritus Ammoniæ fœtidi, gtts. x.
Sumat cochleare unum medium tertiâ quâque horâ.

It is to be observed that much caution is necessary in giving such combinations containing opium to infants, but there is a period when depletion ceases to be useful, that a mixture like this will prove the most effectual means of curing convulsions. In such cases of convulsions, in addition to the use of purgative medicine, prescribing the mother's milk, and giving oil of turpentine, you may, during the first twenty-four hours, while the child is strong, order a warm bath, applying at the same time a sponge dipped in cold water to the head; or, if the child is weak, incline its head over the side of the cradle, and use the cold sponge, and you will find that it will diminish the fit.

Before concluding, let me say a few words on inflammation of the spinal marrow. This disease is closely connected with the subject of neuralgia. *Myelitis* is so liable to be confounded with a great variety of painful affections, that every ascertained case of inflammation of the spinal marrow ought to be recorded for the purpose of rendering more perfect a department of pathology already diligently but not completely cultivated. A young married woman was admitted into the Meath Hospital on the 12th of September, 1838. She was healthy until the period of marriage, soon after which her husband commenced a system of ill-usage, comprising beating, kicking, throwing down stairs, &c., &c. He was frequently drunk, and occasioned her every species of grief. No wonder that a life like this should have reduced our patient to the truly miserable condition she was in. She had been injured so often, that it was difficult to say to what particular act of violence her present malady ought to be referred. She is much emaciated, respirations hurried, and pulse very quick. Has no headache, but complains much of agonizing pains in the loins, aggravated by pressure of the lumbar spinous processes, extending round the abdomen, and downwards to the hips and thighs. There is no pectoral affection, and her tongue, state of stomach, and general appearance are not those of a person labouring under fever. She writhes in the bed from the violence of the pains; she does not sleep night or day, and disturbs the other patients by her cries.

Blood was drawn by cupping from the loins; leeches were applied, and Dover's powder administered. Her extreme emaciation prevented us from adopting either more active depletion by the lancet, or the use of calomel. In short, we sought to relieve, not to cure, for her death appeared inevitable. Blisters we could not apply on account of the great emaciation. On the 15th we found that she had been screeching all night, and constantly wanting extract of opium, which was ordered her as a palliative. On the 16th she complained that the sense of feeling was leaving her thighs, and she died on the 18th, five days after admission. *On dissection* we found all the viscera healthy; there was extreme atrophy of the intestines, especially the colon and cæcum ; so it is probable that starvation was among her afflictions. The lower portion of the spinal marrow and the cauda equina exhibited an excessive vascularity and redness, but no exudation of lymph. Each nervous fasciculus of the cauda exhibited a vein on its posterior surface distended with blood; and the remaining portion of each fasciculus displayed great arterial vascularity.

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